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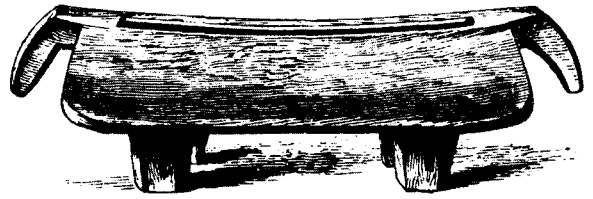
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LANDEG WHITE has taught at the Universities of the West Indies, Malawi, Sierra Leone, and Zambia, and he is now a research fellow at the Centre for Southern African Studies at the University of York. His publications include *V. S. Naipaul: A Critical Introduction* (1975), (with Leroy Vail) *Capitalism and Colonialism in Mozambique: A Study of the Quelimane District* (1980), *For Captain Stedman: Poems* (1983), and (with Jack Mapanje) *Oral Poetry from Africa* (1983). In 1981 he organized the York Conference on Literature and Society in Southern Africa, the proceedings of which are forthcoming. He is currently writing the history of a village in Malawi and is working in collaboration with Leroy Vail on a study of southern African praise poetry.

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The Wars of August: Diagonal Narrative in African History

ROBERT W. HARMS

THE GROWTH OF TEACHING AND RESEARCH on the history of precolonial tropical Africa in the late 1950s and early 1960s presented a challenge to the Western historical consciousness. By ignoring the traditional distinction between prehistory (attributed to oral societies) and history (attributed to literate societies) as well as the distinction between ethnohistory (attributed to tribes) and history (attributed to nations), historians of precolonial tropical Africa implicitly questioned conventional notions of what the discipline of history was all about. Hugh Trevor-Roper reacted strongly in 1963:

If all history is equal, as some now believe, there is no reason why we should study one section of it rather than another; for certainly we cannot study it all. Then indeed we may neglect our own history and amuse ourselves with the unrewarding gyrations of barbarous tribes in picturesque, but irrelevant corners of the globe: tribes whose chief function in history, in my opinion, is to show to the present an image of the past from which, by history, it has escaped.¹

At about the same time, however, some Continental thinkers and some Anglo-American philosophers were rethinking the meaning of history and the Western historical consciousness. The result, according to Hayden White, was the idea that

the historical consciousness on which Western man has prided himself since the beginning of the nineteenth century may be little more than a theoretical basis for the ideological position from which Western civilization views its relationship not only to cultures and civilizations preceding it, but also to those contemporary with it in time and continuous with it in space. In short, it is possible to view historical consciousness as a specifically Western prejudice by which the presumed superiority of modern, industrial society can be retroactively substantiated.²

This essay was originally prepared as a talk for the fellows of the Whitney Humanities Center at Yale University in the spring of 1982. I am grateful to the fellows for their comments and to the center for giving me the opportunity to explore these ideas. As the original talk began to take the form of an essay, I received valuable comments from Leonard Thompson, Jan Vansina, and Jennifer Widner.

¹ Trevor-Roper, *The Rise of Christian Europe* (London, 1964), 9. A similar view was expressed at the 1965 meeting of the American Historical Association; see Leonard Thompson, "African History in the United States," *African Studies Bulletin*, 10 (1967): 54.

² White, *Metahistory: The Historical Imagination in Nineteenth-Century Europe* (Baltimore, 1973), 2–3. White was referring primarily to works by Claude Lévi-Strauss and Michel Foucault and to the positions expressed in William H. Dray, ed., *Philosophical Analysis and History* (New York, 1966).

If history was to be more than the “specifically Western prejudice” so clearly illustrated by Trevor-Roper, and if the Western historical consciousness was to appreciate the diversity of human experience and the full humanity of all people, then the history of precolonial tropical Africa and other neglected areas of the world *had* to be written.

Beyond the philosophical questions lay more mundane and difficult issues. The first was to find data, a problem that not only set off a search in the libraries and archives of Europe and North Africa for manuscripts and documents relating to tropical Africa but that also prompted new interest in the use of oral traditions, archaeology, ethnography, linguistics, serology, and even dendochronology. This combination of techniques prompted Wyatt MacGaffey, an anthropologist, to characterize African history as the “decatheon of social science.”³ Another problem was to determine the most appropriate literary forms by which to transform these data into something recognizable as history. Historians of Africa, like their counterparts in more established subfields, had two main options when constructing a historical work: the conventional *vertical* narrative, which traces a specific theme through a series of permutations over time, or the alternative *horizontal* section, which synchronically reconstructs a variety of institutions and phenomena coexisting in a certain “slice” of time. In practice, the distinction is not always clear cut, but most works can be readily identified as predominantly one or the other. The choice of form has far-reaching consequences, for form determines the kinds of questions asked, the kinds of connections made, and the sorts of boundaries drawn. Moreover, it involves fundamental notions of causality: whether the understanding of an event depends more on an understanding of its antecedents or on an understanding of its position or function within an existing system.⁴ The choice is not simply a matter of individual preference; some types of data lend themselves better to vertical treatment, and others fit more comfortably within the horizontal section.

The data on precolonial tropical Africa are as uneven as they are diverse, but three evidentiary situations can be distinguished. Each has influenced the scope of the history that can be written, and each has been associated with a certain literary form. The first situation pertains to topics on which written documents of the type traditionally used by historians can be found. Such documents were usually generated by trading contacts between the fringes of tropical Africa and the outside world, although in a few areas traders regularly penetrated the interior and, alternatively, many fringe areas were never visited at all. The parts of West Africa just south of the desert had ancient trading contacts with the Mediterranean world,

³ MacGaffey, “African History, Anthropology, and the Rationality of the Natives,” *History in Africa*, 5 (1978): 103. For discussions of various techniques, see Daniel McCall, *Africa in Time Perspective* (London, 1964); and Creighton Gable and Norman R. Bennett, eds., *Reconstructing African Culture History* (Boston, 1967). For an example of linguistic reconstruction, see Christopher Ehret, *Southern Nilotic History* (Evanston, Ill., 1971). In 1974 the African Studies Association established *History in Africa: A Journal of Method* to keep historians abreast of new developments.

⁴ James Henretta, “AHR Forum—Social History as Lived and Written,” *AHR*, 84 (1979): 1299; and Jonathan Culler, *The Pursuit of Signs: Semiotics, Literature, Deconstruction* (Ithaca, N.Y., 1981), 30. Culler has held that “the single most important way of characterizing a piece of research may be to ask whether it is synchronic or diachronic in character”; *ibid.*, 31.

and some of the legacy of these contacts can be found in surviving Arabic documents and manuscripts. As Islam spread in West Africa after the eleventh century, manuscripts of indigenous origin appeared as well.⁵ For the east coast of Africa, which has long carried on trade with the Middle East and India, scattered documents date back to approximately A.D. 120.⁶ Documentation on the western coast of Africa began to accumulate after 1446, when Portuguese explorers reached the mouth of the Senegal River, and it increased steadily through the period of the Atlantic slave trade. For the southern tip of Africa, traders' documents date from 1486, when the Portuguese rounded the Cape, and a special category of documents came into being after the beginning of permanent settlement in the Cape Colony in 1652.⁷

Traders' documents have long been used to write the history of European expansion, but historians of Africa began to use them to shed light on the doings of the Africans themselves. To understand better the unfamiliar African institutions mentioned in the documents, historians often relied on the insights of anthropology. Some scholars supplemented the documentary evidence with evidence from oral tradition. In scope, histories based on these sources deal primarily with trade, diplomacy, and indigenous politics, although some scholars branched out to write social and economic histories set in the era of the slave trade and its aftermath.⁸ Because the data reveal change over time and because they focus on basic themes, such as politics, diplomacy, and trade, books based on them are usually standard

⁵ For English translations of the most important Arabic sources, see Nehemiah Levtzion and J. P. F. Hopkins, eds., *Corpus of Arabic Sources Relating to West Africa* (Cambridge, 1882). The most famous indigenous manuscripts are al-Sa'di, *Tarikh al-Sudan* (ca. 1655), Arabic text and French translation by O. Houdas (Paris, 1911); and Ibn al-Mukhtar, *Tarikh al-Fattash* (ca. 1665), Arabic text and French translation by O. Houdas and W. Delafosse (Paris, 1913).

⁶ G. S. P. Freeman-Grenville, ed., *The East African Coast: Select Documents from the First to the Earlier Nineteenth Century* (Oxford, 1962); and J. E. O. Sutton, *The East African Coast: An Historical and Archeological Review*, Historical Association of Tanzania, paper no. 1 (Nairobi, 1966).

⁷ Monica Wilson and Leonard Thompson, eds., *The Oxford History of South Africa*, 1 (New York, 1969): 75, 184. South Africa formed a somewhat special case because it had permanent white settlers from 1652 and it never supplied slaves to overseas markets. Histories of indigenous inhabitants were based on documents resulting from natives' interaction with settlers as well as on oral sources. See J. D. Omer-Cooper, *The Zulu Aftermath: A Nineteenth-Century Revolution in Bantu Africa* (Evanston, Ill., 1966); Leonard Thompson, ed., *African Societies in Southern Africa: Historical Studies* (New York, 1969); Wilson and Thompson, *Oxford History of South Africa*; Leonard Thompson, *Survival in Two Worlds: Moshoeshoe of Lesotho, 1786–1870* (Oxford, 1975); Richard Elphick, *Kraal and Castle: Khoikhoi and the Founding of White South Africa* (New Haven, 1978); Jeff Guy, *The Destruction of the Zulu Kingdom* (London, 1979); Shula Marks and Anthony Atmore, eds., *Economy and Society in Pre-Industrial South Africa* (London, 1980); and J. B. Peires, *The House of Phalo* (Berkeley and Los Angeles, 1982).

⁸ There is an entire genre of "trade and politics" books. See E. W. Bovill, *Caravans of the Old Sahara* (Oxford, 1933); Kenneth O. Dike, *Trade and Politics in the Niger Delta* (London, 1954); David Birmingham, *Trade and Conflict in Angola* (London, 1966); A. I. Akinjogbin, *Dahomey and Its Neighbors, 1708–1818* (Cambridge, 1967); W. G. L. Randles, *L'Ancien Royaume du Kongo des origines à la fin du XIX^e siècle* (Paris, 1968), and *L'Empire du Monomotapa du XI^e au XIX^e siècle* (Paris, 1975); Alan Ryder, *Benin and the Europeans, 1485–1897* (London, 1969); Kwame Daaku, *Trade and Politics on the Gold Coast, 1600–1720* (Oxford, 1970); Walter Rodney, *A History of the Upper Guinea Coast, 1545–1800* (Oxford, 1970); C. S. Nicholls, *The Swahili Coast: Politics, Diplomacy, and Trade on the East African Littoral, 1798–1856* (London, 1971); Phyllis Martin, *The External Trade of the Loango Coast, 1576–1870* (Oxford, 1972); Nehemiah Levtzion, *Ancient Ghana and Mali* (London, 1973); and H. H. K. Bhila, *Trade and Politics in a Shona Kingdom* (London, 1982). For social histories, see G. I. Jones, *The Trading States of the Oil Rivers* (London, 1963); Kamman K. Nair, *Politics and Society in South Eastern Nigeria* (London, 1972); A. J. H. Latham, *Old Calabar, 1600–1891* (Oxford, 1973); and Frederick Cooper, *Plantation Slavery on the East Coast of Africa* (New Haven, 1977). For economic histories, see Latham, *Old Calabar, 1600–1891*; Philip Curtin, *Economic Change in Precolonial Africa: Senegambia in the Era of the Slave Trade* (Madison, Wisc., 1975); and Patrick Manning, *Slavery, Colonialism, and Economic Growth in Dahomey* (Cambridge, 1982).

vertical narratives. Often what differentiates them from vertical narratives written on Europe and America is the addition of an introductory chapter that presents a panoramic view of the geography and ethnography of the area as a background for the events that occupy the remainder of the book.

The second situation pertains to topics on which little or no written documentation was generated for the period prior to the explorations of the late nineteenth century but for which deep and elaborate oral traditions survive. Most common for the kingdoms of the interior, this situation is not universal for any type of indigenous political structure. Not all kingdoms retained elaborate oral traditions, and some peoples who had no kingdoms retained oral histories of named and bounded corporate groups such as clans.⁹ Because oral traditions not supported by documentary evidence pose a whole new series of methodological problems, the books relying on those traditions often contain extended meditations on the meaning of the traditions themselves. The techniques for collecting and analyzing oral traditions, first elaborated by Jan Vansina in 1961, have since been influenced by the structuralist approaches of anthropologists such as Claude Lévi-Strauss and Edmund Leach. Historians who work with oral traditions have recognized that many were symbolic statements of political or social ideology but have held that their symbolic formulation does not preclude their basis in historical events. Indeed, scholars have argued that, if the symbols and ideologies were themselves seen as historical phenomena, the oral traditions could be used to uncover aspects of social and intellectual history.¹⁰ In either case, it was impossible to know what to make of the traditions without a detailed understanding of the symbols, institutions, and ideologies of the group under study, knowledge that could come only from detailed field work and some familiarity with anthropology.¹¹ In spite of the novelty of the analytical methods, the scope of these books is quite conventional; it is usually

⁹ Jan Vansina, *L'Évolution du royaume Rwanda des origines à 1900* (Brussels, 1962), and *The Children of Woot: A History of the Kuba Peoples* (Madison, Wisc., 1978); Robert Smith, *Kingdoms of the Yoruba* (London, 1969); Raymond Kent, *Early Kingdoms in Madagascar, 1500–1700* (New York, 1970); David Cohen, *The Historical Tradition of the Busoga* (London, 1972); Samwiri Rubaraza Karugire, *A History of the Kingdom of Nkore in Western Uganda to 1896* (Oxford, 1971); M. S. M. Semakula Kiwanuka, *A History of Buganda from the Foundation of the Kingdom to 1900* (London, 1971); Charlotte Quinn, *Mandingo Kingdoms of the Senegambia* (London, 1972); Mutamba Mainga, *Bulozi under the Luyana Kings* (London, 1973); Andrew Roberts, *A History of the Bemba* (London, 1973); Steven Feierman, *The Shamba Kingdom: A History* (Madison, Wisc., 1974); Joseph Miller, *Kings and Kinsmen: Early Mbundu States in Angola* (London, 1976); Ivor Wilks, *Asante in the Nineteenth Century* (Cambridge, 1975); Robin Law, *The Oyo Empire, c. 1600–1836* (Oxford, 1977); David Beach, *The Shona of Zimbabwe, 900–1850* (London, 1980); Roy Willis, *A State in the Making: Myth, History, and Social Transformation in Precolonial Ufipa* (Bloomington, Ind., 1981); and Thomas Reece, *The Rainbow and the Kings: A History of the Luba Empire to 1891* (Berkeley and Los Angeles, 1981). For studies based largely on clan and lineage histories, see Bethwell Ogot, *A History of the Southern Luo* (Nairobi, 1967); Isaria Kimambo, *A Political History of the Pare of Tanzania, c. 1500–1900* (Nairobi, 1974); John Lamphear, *The Traditional History of the Jie of Uganda* (London, 1976); Thomas Spear, *The Kaya Complex: A History of the Mijikenda Peoples of the Kenya Coast to 1900* (Nairobi, 1978); Randall Packard, *Chiefship and Cosmology: An Historical Study of Political Competition* (Bloomington, Ind., 1981); and Paul Irwin, *Liptako Speaks* (Princeton, 1980).

¹⁰ Jan Vansina, *De la tradition orale* (Tervuren, 1961). For discussions of the structuralist-literalist debate, see the essays in Joseph Miller, ed., *The African Past Speaks: Essays in Oral Tradition and History* (Folkestone, Kent, 1980); and, for examples of structuralist approaches, see Willis, *A State in the Making*; Feierman, *The Shamba Kingdom*; Reece, *The Rainbow and the Kings*; and Vansina, *The Children of Woot*.

¹¹ The Fourth International African Seminar, held at Dakar in 1961, passed a resolution advocating the training of researchers in both history and anthropology for precisely this reason; Jan Vansina et al., *The Historian in Tropical Africa* (London, 1964), 51.

political history, dotted with wars and succession struggles. In form, these books are vertical narratives with chronologies based on king-lists, genealogies, or age-set successions. But they generally depart from the vertical emphasis to give a broad geographical and ethnographic introduction and to discuss the implications of the founding myths, which often served as “social charters” for various indigenous institutions.

The third evidential situation has so far captured the interest of a very small number of historians, but it will need much more attention in the future if our knowledge of precolonial Africa is to gain any sort of balance. This situation pertains to topics for which neither deep documentation by outsiders nor elaborate oral traditions exist. It is characteristic of vast areas of Africa where local chiefs and lineage headmen successfully avoided incorporation into large states. These areas were dotted with micropolities in which the kinship structure was often indistinguishable from the political structure, making political history in the normal sense impossible.¹² A similar problem of data exists with regard to kingdoms for all levels of society below the ruling elites.¹³

There are, nevertheless, sources that can be used. Some sense of time depth can be obtained by genealogies, however shallow, and oral traditions, however sparse and sketchy. There is sometimes physical evidence of the past: clearings that have been abandoned, clusters of palm trees that mark the sites of ancient villages, dams and irrigation works constructed by ancestors whose names are sometimes remembered. But the most voluminous sources offer synchronic accounts of life in the late nineteenth or early twentieth century. These include explorer and missionary accounts, which describe a variety of local customs and institutions and which were usually written in the tradition of ethnography rather than history; ethnographic studies by early colonial officials who were trying to determine how to govern the people over whom they claimed authority; and studies by anthropologists trying to recreate “traditional” African life in all its dimensions. In the absence of such sources, interviews with the oldest members of the society can sometimes be used to reconstruct the economic, social, political, and cultural patterns of late precolonial life. This information lends itself to a kind of reconstruction that differs fundamentally in scope from that based on traders’ documents or deep oral traditions: it is broad instead of deep, illuminating a series of activities and institutions that existed simultaneously instead of tracing a single theme over time; it focuses on the activities of common people, seen as a group, instead of elites, often seen as individuals; and it recounts patterned behavior, which was recurrent, instead of events, which were unique. This information is best suited to a kind of historical ethnography that seeks to recreate the institutions and patterned activities of the

¹² The most common way of dealing with such regions has been to concentrate on trade patterns, often inferred from coastal documents. See, for example, Elizabeth Isichei, *A History of the Igbo People* (London, 1976); Edward Alpers, *Ivory and Slaves in East Central Africa* (Berkeley and Los Angeles, 1975); David Northrup, *Trade without Rulers: Pre-Colonial Economic Development in South-Eastern Nigeria* (London, 1978); and Robert Harms, *River of Wealth, River of Sorrow: The Central Zaire Basin in the Era of the Slave and Ivory Trade* (New Haven, 1981).

¹³ See Jan Vansina, *The Tio Kingdom of the Middle Congo, 1880–1892* (London, 1973); and Gwyn Prins, *The Hidden Hippopotamus* (Cambridge, 1980).

society under study.¹⁴ It therefore raises a special set of problems about how best to fit data into a literary form.

There is a precedent for historical reconstruction of such broad scope in the *histoire totale* of Fernand Braudel's *The Mediterranean and the Mediterranean World in the Age of Philip II*.¹⁵ Africanists could find in Braudel a kindred spirit: he wrote about a region that was not, during the period he covered, at the center of world history, and he had an eye for the common instead of the spectacular. He had, however, arrived at his particular approach by a very different path. In defining the analytical scope of his grand *enquête*, he had been particularly influenced by two social sciences that made every aspect of society the proper scope of inquiry: geography, which includes the study of human societies as well as of landscapes, and sociology, the sociology of Émile Durkheim, which seeks to explain human action in terms of larger social patterns instead of individual psychology. Braudel wrote of the "convergence of the two social sciences, history and geography, which find no advantage in being separated from each other."¹⁶ He felt that Paul Vidal de la Blache's *Tableau de la géographie de la France*, published in 1903, was both a major work in geography and "a canon of the French school of history"; and Braudel's mentor, Lucien Febvre, a founder of the *Annales* school, had published *A Geographical Introduction to History* (1925), which prefigured many of the formulations that appeared in *The Mediterranean*.¹⁷ And Braudel once wrote that "sociology and history made up a single intellectual adventure, not two different sides of the same cloth, but the very stuff of the cloth itself, the entire substance of its yarn."¹⁸

Such concerns led Braudel to write history in which the study of "man" nearly squeezed out the doings of individual men. In preparing the second edition of *The Mediterranean*, he included a section dealing with events only after an apology.¹⁹ Events, he felt, were the "ephemera of history." As Samuel Kinser paraphrased it, "events are dust. . . . they are infinite in number; they float here and there, scarcely touching the real soil of history."²⁰ What really interested Braudel were the changeless relationships between people and their environment and the cyclical economic and social trends. Historical reconstruction of such breadth clearly cannot be accommodated by the conventional vertical narrative, which best displays linear successions of events and causes followed by effects. Braudel, in writing *The*

¹⁴ For a guide to this "total" approach to ethnography, see *Notes and Queries on Anthropology* (6th edn., London, 1967).

¹⁵ Braudel, *The Mediterranean and the Mediterranean World in the Age of Philip II*, trans. Siân Reynolds, 2 vols. (New York, 1972–73). The first French edition appeared in 1949; the second, on which the translation is based, in 1966.

¹⁶ Samuel Kinser, "Annaliste Paradigm? The Geohistorical Structuralism of Fernand Braudel," *AHR*, 86 (1981): 66.

¹⁷ Braudel, *On History*, trans. Sarah Matthews (1969; Chicago, 1980), 17; and Lucien Febvre, *A Geographical Introduction to History*, trans. E. G. Mountford and J. H. Paxton (1922; New York, 1925). Braudel dedicated *The Mediterranean* to Lucien Febvre.

¹⁸ Braudel, *On History*, 69. Some French Africanists were influenced by the "total" approach: the historian Henri Brunschwig, who held long conversations with Braudel when the two were in prison camp during World War II, and the geographer Raymond Mauny, whose *Tableau géographique de l'ouest africain au moyen âge* (Dakar, 1961) was a landmark work in West African history, combined their special concerns with broader interests characteristic of the *Annales*. Braudel, *On History*, iv.

¹⁹ Braudel, *The Mediterranean*, 2: 901–03.

²⁰ *Ibid.*, 901; and Kinser, "Annaliste Paradigm," 94.

Mediterranean, chose the horizontal section as the literary form most appropriate to his survey of a vast array of social, economic, political, and cultural institutions of the Mediterranean world between 1550 and 1600.

Africanists, too, have been attracted by the horizontal slice, in part because it provides a framework that can accommodate broad but shallow data. The crucial issue then becomes how the horizontal slice is subdivided. The most common solution has been to organize it in a manner similar to the ethnographer's monograph²¹—segmented, somewhat arbitrarily, into a series of side-by-side discussions of the various phenomena under consideration. This strategy allows an abundance of varied data on a given village, society, or region to be gathered into a single volume. The differences between this approach and the vertical narratives are profound. Narrative histories of precolonial Africa usually provide an ethnographic opening chapter as the setting for history; the horizontal slice offers the panoramic sweep as the stuff of history itself: narrative histories of precolonial Africa set historical data in its ethnographic context; horizontal accounts set ethnographic data in their historical context.

Although horizontal-slice history has a great deal in common with traditional ethnography, the two disciplines differ radically in their treatment of time. Ethnographers have usually written in the “ethnographic present,” which blurs the distinction between past and present to convey the supposed timeless quality of “traditional” or “primitive” society. Historians, by contrast, have most often been interested in placing their data in specific temporal contexts.²² Instead of regarding the horizontal section as a timeless ethnographic present, Braudel saw it as the intersection of three categories of historical phenomena, each with its own rate of change. The structures, such as the relationship between preindustrial people and their environment, remained stable over the long term. In *The Mediterranean* he referred to the history of structures as “a history in which all change is slow” and as “a history that changes little or not at all.”²³ Set against these structures is the history of social and economic trends. Each trend had its own length; depending on its nature, a trend could last for a few years or for centuries. The shortest time span was reserved for events. Each of these three kinds of time formed the basis for one part of *The Mediterranean*.

Braudel's ideas provided historians of precolonial tropical Africa with a way of determining to what extent the findings of present-day field work or early colonial ethnography could be used to illuminate the precolonial past. For Braudel, different categories of historical phenomena had different degrees of stability and, therefore, did not all require the same degree of temporal specificity in their documentation. Because the structures were essentially static, Braudel felt free, in part 1 of *The Mediterranean*, to “make full use of evidence, images, and landscapes dating from other periods, earlier and later, and even from the present day.”²⁴

²¹ For examples of Africanist ethnography, see the more than fifty volumes of the *Ethnographic Survey of Africa* published by the International African Institute (London, 1950–).

²² For Braudel's ideas on the difference between historical time and sociological time, see *On History*, 47–50, 78.

²³ Braudel, *The Mediterranean*, 1: 20, 353.

²⁴ *Ibid.*, 23.

Trends required documentation at intervals frequent enough to establish the pattern of change. Events needed chronologically precise documentation. In the 1970s some Africanists began to employ Braudelian models to deal with the various categories of data that they were collecting. Vansina's *Tio Kingdom of the Middle Congo, 1880–1892*, which is an explicit attempt to erase the distinction between history and ethnography, follows a Braudelian scheme.²⁵ It, too, has three distinct parts: one on structures, in which the results of Vansina's own field work, conducted in the 1960s, are combined with the recollections of aged informants and the writings of nineteenth-century observers to reconstruct the economic and social life of Tio village society; one on trends, primarily patterns of trade, based on the writings of contemporary observers; and, finally, a short one on events. In Gwyn Prins's *The Hidden Hippopotamus* three of the four parts that make up the book are devoted to ethnographic descriptions of life in the Buluzi Kingdom in the late nineteenth century. These descriptions, based partially on field work conducted in the 1970s, are organized around certain "core concepts" that, he argued, belong to "those deeper levels" that remain stable over the long term.²⁶

The horizontal section provides a way of dealing with breadth in historical reconstruction, and it may well be the best framework for certain types of data; but, while solving certain problems, it raises others, which can be illustrated by reference to *The Mediterranean*. In the first place, horizontal-slice history can easily become history without movement, a static construction that deprives the discipline of the dynamic that has been its very essence. Braudel got around this problem by inserting a section on events at the end, thus juxtaposing the horizontal section with the vertical narrative, but in many African cases events are beyond recovery, which would leave only the horizontal picture. One Africanist has recently hailed the Braudelian approach for its rejection of "linear succession" and its concentration on fundamental structures.²⁷ But fundamental structures, in the Braudelian scheme, are essentially static, and excessive concentration on them could result in works in which the "ethnographic present" of the traditional ethnographer becomes the "long term" of the new historian, thus reinforcing the stereotype of a static African past. A second danger in the Braudelian approach is that *histoire totale* can lead to descriptive sprawl.²⁸ New rooms can easily be added to Braudel's historical house. If everything is important, where does one stop? What constitutes a legitimate test of relevance? It is no accident that *The Mediterranean* is over twelve hundred pages long.

²⁵ Vansina added a spacial dimension to the Braudelian scheme, arguing that the geographical distribution of an institution influenced its rate of change; *The Tio Kingdom of the Middle Congo*, 497. The careful attention that Vansina, a historian, paid to time forms a contrast with the assumptions of relative stasis found in historical ethnography written by anthropologists. See, for anthropological examples, Georges Balandier, *Daily Life in the Kingdom of the Kongo from the Sixteenth to the Eighteenth Century* (New York, 1969); and Melville Herskovits, *Dahomey: An Ancient West African Kingdom*, 2 vols. (New York, 1938).

²⁶ Prins, *The Hidden Hippopotamus*, 8.

²⁷ W. G. Clarence-Smith, "For Braudel: A Note on the 'École des Annales' and the Historiography of Africa," *History in Africa*, 4 (1977): 275–76.

²⁸ J. H. Hexter has written, "*Histoire totale*—but what are its bounds? And is 'total history' the correct translation? Should it not rather be 'endless history,' 'interminable history?'" Hexter, "Fernand Braudel and the Monde Braudellien . . .," *Journal of Modern History*, 44 (1972), reprinted in Hexter, *On Historians: Reappraisals of Some of the Makers of Modern History* (Cambridge, Mass., 1978), 107.

The most serious problem is that the Braudelian paradigm, like traditional ethnography, lacks “structure” in the sense of the term as currently used in the social sciences.²⁹ In the conclusion to the second edition of *The Mediterranean*, Braudel acknowledged that his structural history was quite different from the structuralism of social science, which was concerned with finding the underlying logic that linked or ordered seemingly disparate phenomena.³⁰ Braudel’s structuralism is, to use one of his favorite similes, like the structure of a house that provides a variety of floors and rooms to fill with furniture. The scheme creates divisions instead of connections. The relationships among the rooms are not clear. The lack of structural connections in *The Mediterranean* is evident on three levels. At the lowest level, a typical subsection of a chapter explores a pattern of historical behavior: mountain people, for example, are characterized as rough-and-ready, or plains people are seen as civilized. Each pattern is illustrated with several examples. But what is it about mountains that *cause* rough-and-readiness? And what is it about plains that cause sophistication? The essential links are missing. At a higher level, the links between the subsections of a single chapter, or between one chapter and the next, are not very clear; the divisions are side by side, but they do not interact. At the highest level, the structures are only vaguely related to the trends, which, in turn, are only vaguely related to events.³¹ At least one Africanist has praised Braudel’s stress on the “autonomy of different structures” as an antidote to determinism of all kinds,³² but one need not be a strict determinist to appreciate the interrelatedness of historical phenomena.

These problems arise in part because no technique has been developed for constructing the horizontal slice that is the equivalent of the way historians construct vertical narratives by transforming data into chronicle, and chronicle into story.³³

²⁹ For a careful analysis of this point, see Kinser, “Annaliste Paradigm,” 73, 80–89.

³⁰ Braudel, *The Mediterranean*, 2: 1244.

³¹ Braudel himself was uncomfortable about the organization of *The Mediterranean*. “These subdivisions . . . may not be altogether satisfactory to the intellect,” he wrote. “If I am criticized for the method in which this book has been assembled, I hope the component parts will be found workmanlike.” *The Mediterranean*, 1: 354, 21. He has indeed been criticized on precisely this point.

³² Clarence-Smith, “For Braudel,” 276–77.

³³ One recent approach to structuring the horizontal analysis of African societies is based on the model of the lineage mode of production; see Claude Meillassoux’s “The Economy in Agricultural Self-Sustaining Societies: A Preliminary Analysis,” in David Seddon, ed., *Relations of Production* (London, 1978), 127–46, translated from *Cahiers d’Études Africaines* (1960). Also see Pierre Philippe Rey, *Colonialisme, Néo-Colonialisme, et Transition au Capitalisme* (Paris, 1971), 1–215. In this model, the dominant class is made up not of those who control the means of production (it is assumed that land is plentiful) but of lineage elders who, by virtue of their position in the genealogy, control the circulation of food, material wealth, slaves, and brides. By tracing this circulation, scholars can establish structural connections among such diverse institutions as witchcraft, kinship, and production. Moreover, since some institutions usually dominate or determine others, they can be ordered into a hierarchy. Despite its attractiveness, two problems prevent this model from becoming a general approach to the horizontal slice in African history. The first is that, in many small-scale African societies, there is no clear correlation between kin group organization and the pattern of circulation of wealth; see Robert Harms, “Lineage Mode of Production? Land Tenure and Inequality in Equatorial Africa,” paper presented at the annual meeting of the Canadian Association of African Studies, held in Toronto, May 11–14, 1982; and Jan Vansina, “Lignage, idéologie, et histoire en Afrique Equatoriale,” *Enquêtes et documents d’histoire africaine*, 4 (1980): 133–52. The other problem is that the model is basically static; change occurs primarily when the lineage mode of production begins to be “articulated” with the expanding capitalist world-systems. This feature limits the usefulness of the model for dealing with remote areas of precolonial Africa.

First, the elements in the historical field are organized into a chronicle by the arrangement of the events to be dealt with in the temporal order of their occurrence; then the chronicle is organized into a story by the further arrangement of the events into the component of a "spectacle" or process of happening, which is thought to possess a beginning, middle, and end. This transformation of chronicle into story is effected by the characterization of some events of the chronicle in terms of inaugural motifs, of others in terms of terminating motifs, and of yet others in terms of transitional motifs.³⁴

In this approach, spelled out by Hayden White, the kinds of problems that haunt the horizontal section get resolved, or at least mitigated, in the vertical narrative. The arrangement of data in chronological order creates a dynamic history. The creation of a "story-line" provides a test of relevance that can help reduce descriptive sprawl:

Historical *stories* trace the sequence of events that lead from inaugurations to (provisional) terminations of social and cultural processes in a way that *chronicles* are not required to do. Chronicles are, strictly speaking, open-ended. . . . Stories, however, have a discernible form . . . which marks off the events contained in them from other events that might appear in a comprehensive chronicle of the years covered in their unfoldings.³⁵

Finally, the act of transforming the chronicle into a story forces the historian to develop structure, to make explicit relationships between data and the story line and between one datum and another:

The arrangement of selected events of the chronicle into a story raises the kinds of questions the historian must anticipate and answer in the course of constructing his narrative. These questions are of the sort: "What happened next?" "How did it happen?" "Why did things happen this way rather than that?" "How did it all come out in the end?"³⁶

What gives the vertical narrative its utility is the use of *process* as a structuring principle. Process is a versatile tool. Although it has normally been applied to the conventional succession of events, it can also be used in dealing with complex social phenomena. Victor Turner, an anthropologist, recognized this when he wrote in 1957, "My aim was to show how the general and the particular, the cyclical and the exceptional, the regular and the irregular, the normal and the deviant are interrelated in a single social process."³⁷

Yet this valuable tool is almost out of the reach of historians working with broad but shallow data. The first problem is that the vertical narrative is best suited to dealing with a single process, and this feature limits its analytical breadth. Even the complex processes envisioned by Turner fall far short of *histoire totale*. The second, and more crucial, problem is that process requires sustained vertical movement, which is difficult to reconstruct with shallow data. There are several well-known

³⁴ White, *Metahistory*, 5. Because White made a distinction between story and plot, he could argue that, although horizontal-slice history does not have a story, it does have a plot.

³⁵ White, *Metahistory*, 6.

³⁶ *Ibid.*, 7.

³⁷ Turner, *Schism and Continuity in an African Society* (New York, 1957), 328. Turner organized his analysis in terms of "social dramas" that, he argued, enable the analyst to "observe the crucial principles of the social structure in their operation, and their relative dominance at successive points in time"; *Schism and Continuity in an African Society*, 93. Some sociologists have recently rediscovered the utility of process. See Philip Abrams, "History, Sociology, and Historical Sociology," *Past & Present*, no. 87 (1980): 3–16.

techniques for wringing diachronic movement out of primarily synchronic data, but they all have limited usefulness. One way is by comparative linguistics: the vocabulary relating to a certain institution may be discovered to be borrowed from another language, or the vocabulary of two groups may turn out to be variations on a theme, indicating a common past. Comparative ethnography can be employed in similar ways to reveal the adoption or loss of a certain custom or institution.³⁸ Sometimes, old versions and new versions of oral traditions exist side by side, and the differences or contradictions allow scholars to infer fundamental social, political, or ideological change. Each of these methods may be used to reveal change, but none can be used to reconstruct process; a dichotomy between one time and another is not the equivalent of a discrete series of steps that led from one to the other.

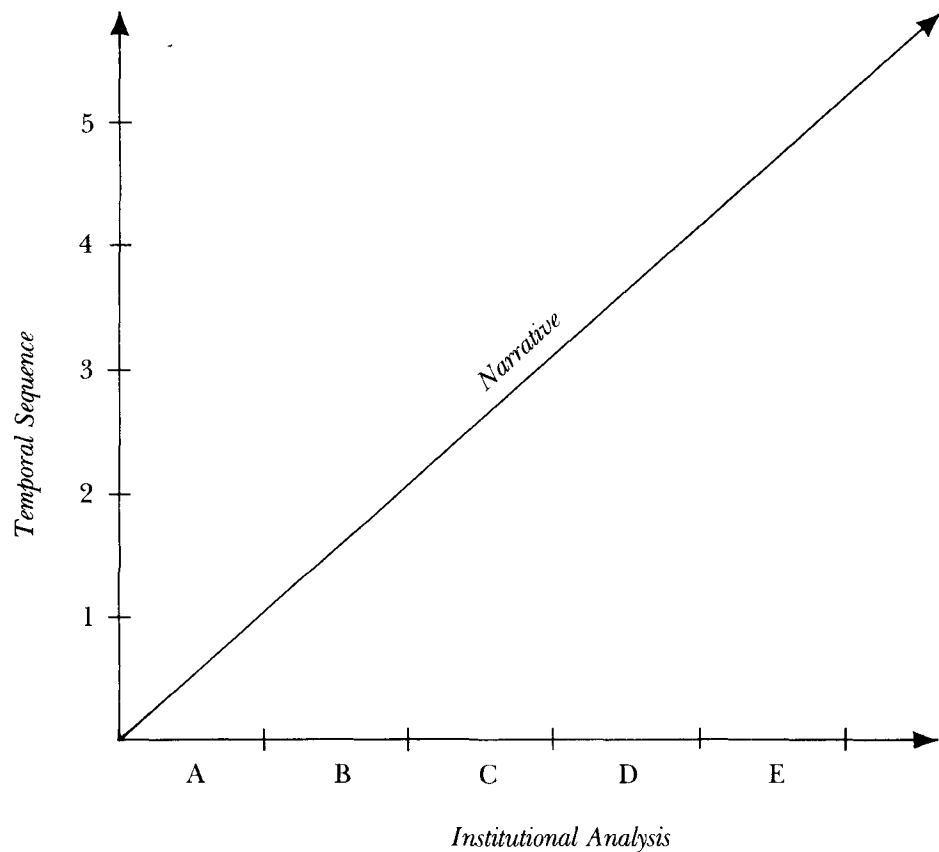
It may, nevertheless, be possible in such situations to combine the structuring capabilities of the vertical narrative with the breadth characteristic of the horizontal slice by constructing a *diagonal* narrative—a combination of a “horizontal” analysis of interrelations among societal institutions and “vertical” movement through time. Given a situation in which the recoverable history of any single institution is described by a simple dichotomy, a narrative must of necessity treat a cluster of institutions and focus on changing relationships within that cluster. It is sometimes possible to identify a change in institution *A*, which provoked an alteration in institution *B*, which in turn caused changes in *C*, and so forth. The resulting narrative is diagonal because it moves through time as it moves analytically from institution to institution.

In contrast to the vertical narrative, which is constructed from diachronic data by first arranging the data in chronological (vertical) order and then selecting certain elements to create a story, the diagonal narrative is constructed from predominantly synchronic data by first establishing the precise links among the institutions to be considered and then determining the temporal sequence of the changes. The institutional links are horizontal, the temporal sequence is vertical, and the resulting narrative, like the vector in a high-school physics problem, is diagonal. Such narratives permit reconstructions of broadly based, sociocultural change that retain many of the attractive features of the horizontal section, while minimizing the dangers. They permit the writing of “history without events” without lapsing into “history without movement,” and they allow for a form of *histoire totale* (or, at least, *histoire abondante*) while minimizing descriptive sprawl. The diagonal narrative is, moreover, structured history because the links among the institutions and the sequence of change lie at the heart of the analysis.

BY WAY OF ILLUSTRATING HOW PREDOMINANTLY SHALLOW DATA can be used to create a diagonal narrative, I shall examine the history of the Nunu, who live on the southern fringes of Zaire’s equatorial forest. This region represents, in the minds of most Americans, quintessential Africa. Despite the progress that has been made in

³⁸ For a discussion of comparative linguistics and comparative ethnography, see Jan Vansina, “Towards a History of Lost Corners in the World,” *Economic History Review*, 2d ser., 35 (1982): 165–78.

FIGURE 1



reconstructing the history of other parts of Africa, the historiography of the equatorial forest in the precolonial period has progressed little beyond the images conjured up by Henry Morton Stanley in *In Darkest Africa* and Joseph Conrad in *Heart of Darkness*—titles that reflect European ignorance and prejudice more accurately than African reality. And we need only glance at the most recent historical syntheses for confirmation.³⁹

This situation has arisen partly because the independent-minded farmers and fisherfolk who inhabited the area successfully resisted any attempts by elites to create large-scale political units and partly because the dominant ideology of the region caused the inhabitants to see their social and political history as an unchanging cycle of alternating generations.⁴⁰ Aside from the rise of slave-trading networks, there were few of the types of events, processes, or trends that historians

³⁹ The most recent general history of precolonial central Africa devotes only a few pages to the forest peoples; David Birmingham, *Central Africa to 1870* (Cambridge, 1981), 47–48. Roland Oliver and Anthony Atmore's synthesis of African history from 1400 to 1800 devotes only three paragraphs to the equatorial forest; Oliver and Atmore, *The African Middle Ages, 1400–1800* (Cambridge, 1981), 22, 117–18. The current situation will change in the next few years when Jan Vansina's multi-volume history of the peoples of the equatorial forest is completed. Vansina is piecing together the history from a variety of historical, ethnographic, and linguistic sources, but his main hindrance is the paucity of historical microstudies of forest peoples.

⁴⁰ Vansina, "Lignage, idéologie, et histoire," 133–40.

generally consider significant: no rising kingdoms, conquering armies, or expanding militant religions. In contrast to the deep genealogies and heroic traditions of the southern savanna, the oral traditions of the forest tell largely of small-scale, short-distance migrations that, when plotted on a map, form a maze of comings and goings. Then why bother with the area at all? In justifying his decision to study the Mediterranean world in a period of its decline, Braudel wrote, "It is my belief that all the problems posed by the Mediterranean are of exceptional human richness, that they must therefore interest all historians and non-historians."⁴¹ The equatorial forest, too, is an area of exceptional richness, all the more because it manifests itself in ways that to us seem so strange.

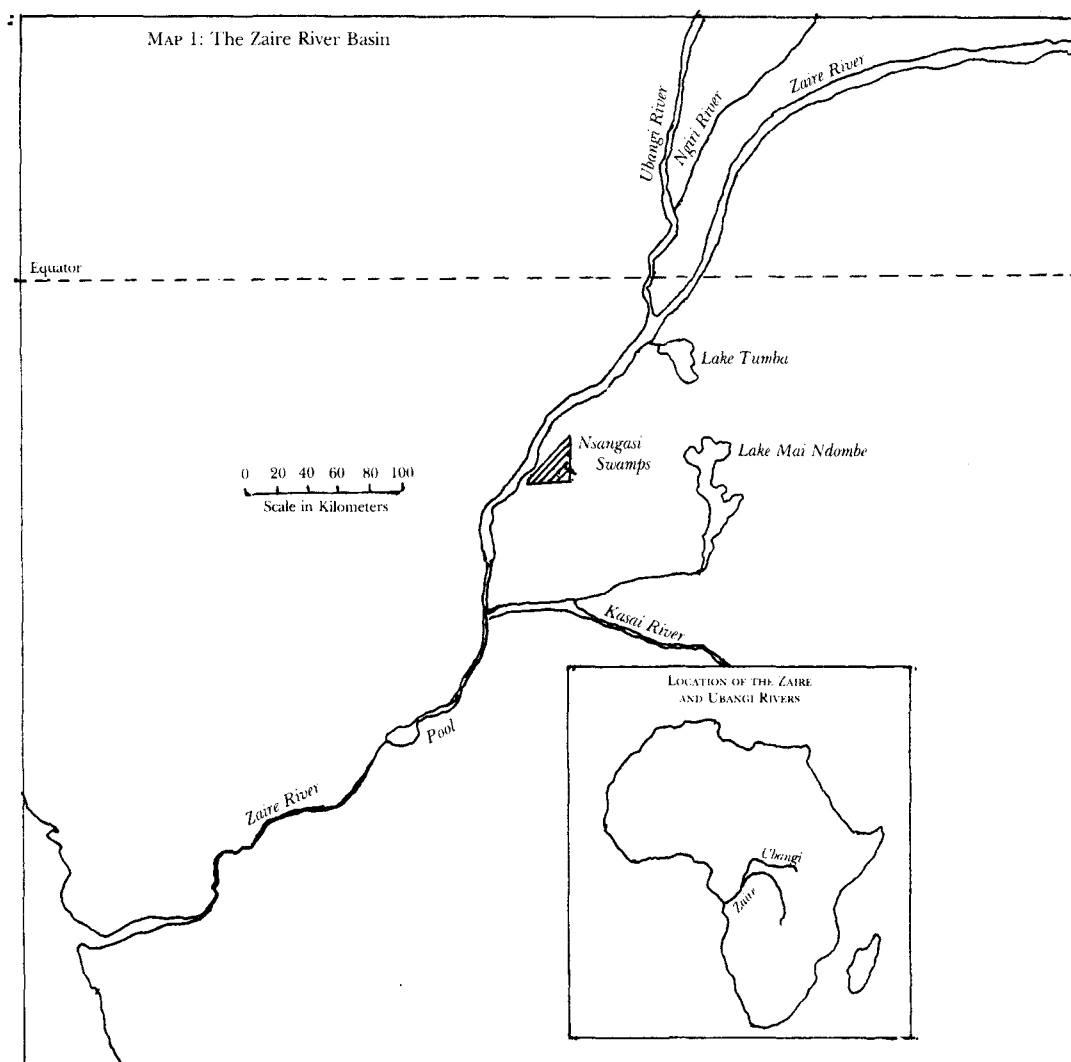
Most of the Nunu are fisherfolk who live in the Nsangasi swamps, which are formed by the floodplain of the Zaire River. The swampland is divided into two ecozones: the flooded forest to the east and the flooded grassland to the west. Since the nineteenth century at the latest, Nunu populations have lived outside the swamps as well. Some of the Nunu are dryland farmers to the east; others are river fishermen living to the southwest.⁴² The oral traditions of the Nunu of the Nsangasi swamps are rather typical of those told all over the forest. Those that deal with the Nunu as a group are very short and focus exclusively on the identification of migration routes. These traditions indicate that the ancestors of the Nunu came from the Ngiri basin, about four hundred kilometers to the north, via the Zaire River.⁴³ Dating the beginning of this migration is impossible, but available genealogies suggest it occurred no later than the mid-eighteenth century, perhaps much earlier. In addition to the general oral histories, there are scattered stories about local wars, and there are family traditions, which concentrate on short-distance moves within and around the swamps. These histories trace a complicated series of movements that can be divided into two historical stages: an early stage that resulted in the peopling of all parts of the swamps, and a later stage characterized by outward movements into the zones surrounding the swamps.

The migration stories in themselves tell little of significance about the history of the Nunu, but they do raise fundamental questions about the historical dynamics of the region. Why were people constantly moving? What reasons compelled them to forsake familiar ecozones for unfamiliar ones? What were the results of such moves

⁴¹ Braudel, *The Mediterranean*, 1: 19.

⁴² Bonzemba, who was born in the early twentieth century, was of the fourth generation of people who had lived east of the swamps. Her ancestor five generations back had been a swamp dweller. Field notes, December 1975. Calculating from genealogies collected in 1934, L. Reynaert placed the founding of the Zaire River village of Bolobo by emigrants from the swamps at around 1840; "Histoire succincte des populations constituant l'actuel sous-chefferie de Bolobo" (1934), *Dossier Politique*, Mushie Zone Archives, Mushie, Zaire.

⁴³ Various versions of the migration tradition are not in total harmony. The Ngiri is cited as the original homeland of the Nunu in Vincent Bokono, "Monkama mo Mibembo mi Bandoko of Ebale e Congo" (typescript; Bolobo, 1960); Lila Ndu'e Bamani, "Ekumela e Banunu-Bobangi" (manuscript, n.d.); and Fedor Bompinda, "Histoire Banunu-Bobangi" (manuscript, 1975). A war song collected in 1975 was more vague, citing "mutu mo ese," the peninsula on which the Ngiri River is located; personal interview, Mongomo, December 13, 1975. Other sources cite a place called Moongo, which is actually a play on words, indicating simply "solid ground." F. Gustin believed that the Nunu came from a number of places; Gustin, "Les Populations de la Mpama-Kasai" (1924), *Dossier Politique*, Mushie Zone Archives, Mushie, Zaire. Given the extreme mobility of the river people, it seems likely that the ancestors of the present-day Nunu were of diverse origins, coming mostly from the Ngiri and surrounding regions. This qualification does not alter the analysis presented here.



on economic, social, and political institutions? The stories suggest that the key dynamics are to be found in the changing relationships between people and their environment. Contrary to the view of Braudel, who treated these relationships as the immovable structures of history, they are more fruitfully seen as a lively arena for human creativity and struggle. Le Roy Ladurie has provided a more appropriate model in *The Peasants of Languedoc*, as Braudel himself acknowledged:

This is the great value of the pioneering work of Emmanuel Le Roy Ladurie on the peasants of Languedoc between the fifteenth and the eighteenth centuries. Who would have thought that this rural order was to such an extent the result of a combination of social, demographic, and economic circumstances and therefore constantly subject to change and alteration?¹⁴

The question is equally relevant to the history of Africa's equatorial forest.

The social, demographic, and economic circumstances of life in the Nsangasi

¹⁴ Braudel, *The Mediterranean*, 1: 28.

swamps before the inevitable changes of the colonial period had to be reconstructed by field work to provide the synchronic baseline from which to identify institutional changes and infer processes. Fortunately, this reconstruction was possible because European-induced change came relatively late to the Nsangasi swamps.⁴⁵ Although the area formally became part of the Congo Independent State in 1885, it was remote. Little direct change resulted from colonial control until 1926, when the colonial government consolidated the Nunu villages and established administrative chiefdoms. Therefore, people born in the swamps around 1900 were teenagers or adults before life changed significantly from its nineteenth-century patterns. These people recounted their personal observations and augmented them with stories heard from their parents and grandparents. Their information provided the ethnographic data on which this analysis is based.

Synchronic data on Nunu institutions of the early twentieth century can be given a diachronic dimension by using comparative ethnography to identify a significant change that resulted from the migration by the ancestors of the present-day Nunu to the Nsangasi swamps. During routine field work—collecting varieties of the dance, the most visible form of artistic expression among the Nunu—it struck me as odd that, out of six ritualized dance forms commonly remembered, four were directly related to war:

1. MBIOKI: Dance on the way to battle and in rejoicing the death of a soldier in battle.
2. NKAKA: Dance before leaving for battle.
3. MOSETO: Dance before a battle and at a funeral.
4. LIMBONGO: Dance to praise those who had died in battle.
5. MONYONDA: Social dance, unrelated to war.
6. LONYONGO: Social dance, unrelated to war.

The Nunu clearly had a warrior cult, in the sense of their heavy emphasis on prowess in war and of their martial values in artistic expression and public ceremonies. To complement the rituals, the Nunu retain tales of warfare, mostly about wars among themselves. This emphasis on war formed a contrast with the swamp-dwellers of the Ngiri valley, the area from which the Nunu claim to have emigrated. Pierre van Leynseele, an anthropologist who has worked in the Ngiri area, has argued that, although the fishing people there defended themselves against slave raids by outsiders, internecine warfare was rare.⁴⁶ Comparative ethnographic data thus suggest that the Nunu warrior cult was not a “primitive” institution dating from time immemorial but something that developed after the ancestors of the present-day Nunu moved to the Nsangasi swamps.

⁴⁵ The field work on which this essay is based was conducted during 1975–76, on a grant from the Social Science Research Council and the American Council of Learned Societies. Subsequent work was done in July and August 1981, with the aid of grants from Yale University’s Concilium for International and Area Studies and the Griswold Fund. I alone am responsible for the conclusions reached here. Tapes of most of the interviews have been deposited in the Archives of Traditional Music, Indiana University, Bloomington.

⁴⁶ Van Leynseele, “Les Libinza de la Ngiri” (Ph.D. dissertation, University of Leiden, 1978), 250.

If this is so, then the warrior cult must have been in some way related to the unique conditions of life in the Nsangasi swamps. This hypothesis can be explored by identifying functional relationships between the warrior cult and other Nunu institutions. Functional analysis has had a checkered past in the hands of historians and anthropologists who believed that, if scholars can identify the benefits of a certain custom or institution, they can explain its function, and, if they can identify its function, they can account for its ultimate origin.⁴⁷ This type of reasoning might go as follows:

- A. Department store sales increase at Christmas time.
- B. The function of Christmas is to increase those sales.
- C. Christmas is a creation of the department stores.

A major fallacy of this reasoning lies in the fact that an institution can serve more than one function, and it may therefore pick up new functions over time, while perhaps losing old ones. While functional analysis can help explain why an institution survives, it cannot by itself explain why an institution originated. A. R. Radcliffe-Brown, the anthropologist, was so annoyed by theories about origins reached on the basis of functional analysis that he declared functionalism irrelevant to historical reconstruction.⁴⁸ But a more limited application of functional analysis does have greater validity. Starting from the verifiable fact that department stores both promote and profit from Christmas, one can argue that, whatever the origins of Christmas and whatever the origins of department stores, at some point in the past the two developed a symbiotic relationship that affected the nature of both. This type of analysis sets up historical propositions that are more easily defended and that suggest new lines of historical inquiry.⁴⁹

At issue here is not the origin of war dances *per se* but rather the functional relationships between the particular warrior cult that existed in the late nineteenth century and other institutions in the Nsangasi swamps. The most obvious relationship was that between the dances and the wars remembered in oral traditions. Some of the wars had been the result of invasions by neighboring groups, but most were among the Nunu themselves. These latter were of two kinds. The first kind were small wars, somewhat on the scale of family feuds. They resulted from local disputes and did not end until the dispute was resolved. The second kind of war was more unusual. These were the wars of August, fought during the dry season when the fishing was over and the swampland water levels were low. The wars of August were institutionalized; they were not fought for territory or to end disputes. The parties were always the same: in the swamps the villages of the flooded

⁴⁷ Robert Nisbet, *Social Change and History* (London, 1969), 228–39.

⁴⁸ Radcliffe-Brown, *Structure and Function in Primitive Society* (New York, 1952), 1–3, 15.

⁴⁹ The type of functional analysis proposed here is somewhat similar to the “consequence statements” advocated by Gerald Cohen in his excellent discussion of the philosophical basis of functional explanation; see Cohen, *Karl Marx’s Theory of History: A Defense* (Princeton, 1978), 249–77. For an attempt to demonstrate a functional relationship between ritual and ecology among a New Guinea people, see Roy A. Rappoport, *Pigs for the Ancestors* (New Haven, 1968). For a defense of his method, see Roy A. Rappoport, “Ecology, Adaptation, and the Evils of Functionalism,” *Michigan Discussions in Anthropology*, 2 (1977): 138–90.

grasslands attacked the villages of the flooded forest, and in the inland region on the eastern edge of the swamps the southernmost villages fought their neighbors to the north. Although slaves were sometimes captured, these were not slave raids but serious tests of strength in which significant numbers of people were killed. Lopanda emphasized the institutionalized character of these wars:

In the eighth month people would go to Monkunungu, the war leader of this area, and ask, "When and where is the war?" And he answered, "We will go to battle on such and such a day." When the day came he blew the antelope horn, "Dee, dee, dee, dee." Everybody came. They said, "Papa, what is the plan? Where shall we go?" He would reply, "The plan is thus."⁵⁰

Why were these wars fought? The person the most knowledgeable on this subject was Lila Ndut'e Bamani, whose name, which translates as "the leader of the warriors," was given him after his grandfather, who had held the title in the nineteenth century. Lila had a very simple answer:

There were too many people. The people saw that the land was crowded, and they decided that they must send some people to be killed in wars. If people died from disease, the people became upset [because disease was attributed to witchcraft], but if soldiers died in war, the people rejoiced.⁵¹

This view of the wars as killing grounds was supported by Yasi, who said that the only thing he knew about the famous war leaders of the past was that they had been responsible for the deaths of a lot of people.⁵² This Malthusian view of Nunu warfare bears a striking resemblance to a theory espoused by Marvin Harris, an anthropologist who believes that war has long been a standard method of population control in hunting and gathering societies, which need to maintain a balance between the human and animal populations: war led to a warrior tradition, which glorified males, thus permitting female infanticide, which kept down the number of reproducers and, hence, the population.⁵³ This theory, with its tortuous reasoning, does not apply to the Nunu in any precise way; no one remembered female infanticide, and no evidence suggests that the ecological balance was ever endangered. But Harris's reasoning does not have to be accepted to recognize the possibility of a functional relationship between war and population control. If Harris, the anthropological theorist, and Lila Ndut'e Bamani, the "leader of the warriors," both saw a link, it is worth asking if and in what sense the area might have been overpopulated.

Certainly the region was not overpopulated in any *absolute* sense—that is, containing more people than the land could support. In 1909 a census of a two-hundred-square-kilometer portion of the flooded grasslands revealed 255 people, or approximately 1.3 people per square kilometer.⁵⁴ The swamps produced such

⁵⁰ Lopanda, personal interview, July 10, 1981.

⁵¹ Lila, personal interview, July 11, 1981.

⁵² Yasi, personal interview, July 13, 1981.

⁵³ Harris, *Cannibals and Kings* (New York, 1977), 33–43.

⁵⁴ "Chefferie Banunu," *Dossier Politique*, Archives of the Equator Region, Political Section, Mbandaka, Zaire. The population in 1909 may have been unusually low as a result of epidemics in the 1890s. Even if the population in 1880 was twice as large as that recorded in 1909, the area could not be considered overpopulated in the nineteenth century.

an abundance of fish that during the colonial period the people of the Nsangasi swamps were important suppliers of smoked fish to Leopoldville.⁵⁵ Yet there appears to have been *relative* overpopulation resulting from an inequitable distribution of resources that caused shortages among certain segments of the population.

One indication of such shortages comes from an analysis of the feuds that continually plagued the area. They were fought mainly over two issues: fishing rights and women.⁵⁶ The issues are closely related; people who obtained fish could sell them in the markets for the small *nsi* shells that served as the local currency, and these shells were an essential part of the bridewealth a young man paid to the family of his intended bride. A man who had access to fish could procure a bride, but a man who lacked such access ran the risk of remaining unmarried unless he found a patron or took drastic action, such as kidnapping a girl from a nearby village, an act that was certain to provoke a small war.⁵⁷ His other option was to try to seize the fishing grounds of a rich man, an action that inevitably resulted in warfare. The wealthy had a war song that encouraged them to defend their property:

Retreat! Advance! To whom will I leave the land
I inherited from my ancestors?
To Strangers? To enemies?
I must fight to protect my birthright.

The small feuds were symptomatic of a society divided between wealthy men, who held rights in productive fishing grounds and used the wealth thus procured to arrange marriages for themselves and their sons, and poor men, who lacked fishing rights and were sometimes forced by circumstances to remain unmarried. There was thus overpopulation in the sense that, given the actual distribution of fishing grounds, there were more potential fishermen than available spots.

The wars of August, I suggest, were similarly related to the tensions between the landless poor and the rich, although the two groups viewed the wars from very different vantage points. The poor not only participated in these wars but provided leaders and heroes. Monkunungu, the best-known war leader of the late nineteenth century, is remembered as a poor man.⁵⁸ Men such as he, who could never hope to gain prestige in Nunu society through the accumulation of wealth, clients, and wives, could become war heroes whose names would be invoked by future generations. Such aspirations were reinforced by the rituals of war. The night before leaving for a battle the warriors danced the *moseto*, the *nkaki*, or the *mbioki*, in which the heroic deeds of their predecessors are recalled. After the war, the people gathered to dance the *limbongo* and the *mbioki* (which literally means "rejoicing") in praise of those who had just died and of ancestors who had died in earlier battles.

⁵⁵ T. J. DeWeerd estimated in 1957 that a good fisherman in the Nkuboko region of the swamps sold twenty-five to thirty-five baskets of smoked fish per year. Each basket was approximately 3 feet by 18 inches by 18 inches; DeWeerd, "Rapport annuel au commissaire de District du Lac Leopold II" (1957), reprinted in J. J. Lingwambi, *Histoire Banunu-Bobangi* (mimeographed; Leopoldville, 1961), 42. One kilogram of smoked fish represented approximately three kilograms of fresh fish; "Rapport annuel," *Régie des Pêcheurs* (Mushie, 1948), Mushie Zone Archives, Mushie, Zaïre.

⁵⁶ Lopanda, personal interview, July 10, 1981.

⁵⁷ Lila, personal interview, July 17, 1981.

⁵⁸ Lopanda, personal interview, July 10, 1981.

The funerals of the dead were the occasion for dancing, once again, the *moseto*. It is indicative of the climate of the times that the main funeral dance of the Nunu was a war dance in which the feats in warfare of the deceased are recounted. A missionary who observed the *moseto* in 1890 vividly described it: "The man in the feather headdress is about to commence dancing and to show how the deceased used to fight in times of war and tell how brave he was. . . . Women also entered the ring and danced and sang of his beauty and strength, and extolled his bravery."⁵⁹ All of this was heady stuff for young men who under ordinary circumstances could look forward to little beyond life at the lowest level of society, and they heeded the call of the antelope horn when it sounded for war. Some became heroes, either through victory or through death.

Sons and nephews of the wealthy also risked their lives in the wars, but many of them had little more to look forward to than had those from poor families. Younger sons or younger nephews did not inherit the fishing grounds of their fathers and uncles; they could, at best, become junior partners, with a proportionately smaller share of the catch, or they could, at worst, be squeezed out to join the landless poor. Becoming a hero in war was a way to gain status that was otherwise denied them. Also operating was what can best be described as a "push factor." The Nunu believed that the wealthy maintained their position with the aid of invisible spirits who demanded payment for their services in human souls. The wealthy man designated a junior kinsman, most commonly a nephew, who eventually contracted a disease and died. In periods of competition for resources, witchcraft accusations were rife, and expendable young people felt especially vulnerable to the power of the rich.⁶⁰ Thus, the Nunu believed it was better for a man to die in war as a hero than to die of disease as a victim of witchcraft.

If the short-term gains of war accrued to the poor and the young, the long-term gains went to the rich and the old. Wealthy men were usually older people who did not fight in the wars. They had become wealthy by inheriting fishing grounds from a father or an uncle and using the proceeds to amass wives and clients. All of this took many years. For them, the wars offered several benefits. In the first place, some of the landless young men who covetously eyed their daughters and their fish-dams might be killed. Given the small total population of the swamps in the late nineteenth century, the killing of even a few landless young men necessarily relieved some of the tension.⁶¹ One's own sons or nephews might also be killed, to be sure, but even this relieved some of the tension over the heir to the fishing grounds. The wealthy were constantly accused of killing their sons and nephews by witchcraft, and so the act of sending a young kinsman to possible death in war seemed relatively benign by comparison. The wars were also important to social

⁵⁹ George Grenfell, "A Funeral Dance at Bolobo, Upper Congo River," *Missionary Herald*, December 1, 1890, p. 450.

⁶⁰ These beliefs were common throughout the area; see Harms, *River of Wealth, River of Sorrow*, 197–215.

⁶¹ The first complete census of the swamps was taken in 1928, after the consolidation of the villages. It showed 721 adult males in the swamps and dryland region to the east. If 60 percent of them were fit to fight, then 432 warriors were available to participate in the various battles "Chefferie Banunu," *Dossier Politique*, Archives of the Equator Region, Political Section, Mbandaka, Zaire. The epidemics of the 1890s and the Spanish Flu epidemic of 1918–19 probably reduced the population considerably. The population in the 1880s may well have been twice as large.

solidarity: they focused the attention of the entire society on the enemy without instead of the tensions within, and they gave to the poor glory and status as partial compensation for their otherwise lowly standing. The wars, in short, mitigated the tensions resulting from the unequal distribution of resources. These and other self-interested motives were well hidden under the rhetoric of patriotism, courage, and strength, which masked the contradictory reasons why the wars had the support of both rich and poor.

The underlying issue, then, was not war but fishing rights. Why was land divided in such a way that there was not room for everyone? The answer lies in the unique nature of fishing in the area. The swamps were part of the floodplain of the Zaire River, which overflowed its banks every November to inundate areas up to twenty kilometers inland. The fish escaped the barren channels of the Zaire by following the surging waters into the flooded forests and grasslands, where they fed on larvae and insects, and where they spawned in December. In April the water began to recede. At first, a solid sheet of water made a giant lunge toward the main river, but soon the tops of the low hills that characterized the grasslands began to appear, forcing the water to flow in well-defined channels. Not until the water got relatively low did the fish begin to follow it back to the Zaire River, and, by then, they found themselves trapped behind the low dams that stretched across the valleys. During the high water in November the fish had entered the grasslands by swimming over the dams, but on the return trip in much lower water they were forced to swim through holes the fishermen had built into the dams. Behind these holes the fishermen placed wicker traps, and thus nearly every fish still in the grasslands ended up in a trap or flopping helplessly in a shallow pool behind a dam. This was not fishing in the normal sense; it was fish-farming with an annual harvest. The dams were extremely efficient, and a single person could harvest all of the fish trapped by one dam simply by emptying the wicker traps every morning. Dam owners claimed the area behind their dams for one to two kilometers along the channel, and they jealously guarded the area against any interloper who tried to divert some of the water and some of the fish.

Fishing in the flooded forest was very different, but it operated on a similar principle. Dams were not feasible in the forest because the terrain was rougher and the trees and bush diverted the water into scores of small channels. Instead, the fishermen dug large ponds in low-lying areas. As the water receded, the fish sought the deepest pools and migrated toward the artificial ponds, where they were trapped when the water got too low. The owners of the ponds had only to throw out the water and scoop up the fish. Like the dam owners, the pond owners claimed as much of the area as possible that fed into their ponds, and they also guarded against encroachments from landless people who wanted to construct ponds near their own. Fishing rights depended on ownership of a dam or a pond and hegemony over the areas they drained. The shortage of land for the poor was a result of the large estates held by the rich, a situation that was, in turn, made possible by a technology that allowed a single person and perhaps a few helpers to harvest all the fish from an area of four square kilometers or more. And this technology was intimately related to the geography of the floodplain itself. The

swamps were, in essence, an enormous fish trap, and the fishermen had learned to exploit them in a manner so efficient that even today it has changed hardly at all.

The analysis presented so far has constructed a horizontal chain of functional relationships linking ritual, warfare, scarcity, class structure, technology, and environment. To become a narrative it needs to include the elements of human choice and chronology. Choice is necessary to create a "story" rather than a set of mechanistic relationships. It can be brought into the analysis by recognizing that these relationships did not arise spontaneously; on the contrary, each was the result of choosing one option out of several that were available. There are probably a hundred possible ways to fish in the swamps, and the methods the Nunu chose favored individual efficiency over equality of opportunity. Land could have been communally owned, as it is in many parts of Africa, but the Nunu developed a system of individual ownership. Inequality can be mitigated in a variety of ways, and the wars of August represent an extreme method. By asking who made these choices and why, anthropology gets transformed into history.

The other necessary element is chronology, which in this case must be relative chronology because precise or even approximate dates are beyond recovery. Although the functional relationships existed simultaneously in the late nineteenth century, the period reconstructed by field work, they did not all come into being at the same time, and the choices that underlay them were not made simultaneously. The choices have to be chronologically ordered, and this ordering process transforms a horizontal chain into a diagonal narrative. If the functional analysis just presented is taken as valid, then it becomes clear that certain functional relationships are predicated on prior relationships. The warrior cult is predicated on institutionalized warfare; warfare is predicated on scarcity of resources, which in turn is predicated on control of resources by the dominant class, a situation predicated on a certain technology adapted to a certain environment.

The historical order, then, is just the opposite: environment, technology, class, scarcity, warfare, ritual.⁶² The diagonal narrative that follows deals with the phenomena in that order, moving laterally from institution to institution while moving forward in time. I do not intend to propose a hypothesis about the order of appearance of the *general* phenomena of war, fishing, dances, and the like, but I am positing the order in which these institutions took a certain form in Nunu society as a result of developing specific functional relationships with other institutions. Warfare, for example, has long existed. It becomes important to the analysis only when it is related to class tensions and takes a form that helps to mitigate those

⁶² The method of ordering employed here differs significantly from that used by Herbert Spencer and other nineteenth-century scholars to construct the cultural history of the entire world. Their method contains a serious flaw in that it assumes that institutional change is progressive, evolutionary change by which societies moved from "primitive" toward "modern." All known institutions could therefore be ranked on a scale from most "primitive" to most "modern," supposedly corresponding to an actual sequence of historical change. Of course, the rankings were arbitrary, reflecting the prejudices of nineteenth-century Europe more accurately than any actual past. Institutions deemed simple were often simply misunderstood, and the existence of a great historical Peter Principle at work, causing everybody to seek ever more cumbersome institutions, has never been proved. In fact, sometimes change occurs in the direction of greater simplicity. See Nisbet, *Social Change and History*, 189–208; and Jack Hirschleifer, "Evolutionary Models in Economics and Law: Cooperative vs. Conflict Strategies," paper prepared for the Liberty Fund, Inc., Seminar on Evolutionary Theory in Law and Economics, 1980, p. 18.

tensions. Similarly, fishing technology has long existed, but it becomes important to the narrative when it takes a certain form in relation to the environment of the Nsangasi swamps. The *test* of the historical order here posited is whether it is consistent with the historical dynamics of the region as spelled out by the oral traditions and genealogies. These sources indicate a gradual movement from the western edge of the swamps to the eastern edge and an eventual spillover to the surrounding ecozones. I hold that the process of institutional change explains that movement, and *vice versa*.

Nunu migration traditions hold that when the first immigrants entered the Nsangasi swamps, the area was uninhabited. This is not surprising, because swamp life, with its flooded landscapes and swarming mosquitoes, is almost intolerable to anyone not accustomed to its peculiarities. The immigrants were people from the Ngiri area who were heading downstream in search of new land for fish-farming. The swamplands of the Nsangasi were remarkably similar to those of the Ngiri, with one important difference: whereas people from the Ngiri could always emigrate downstream in search of new swamplands, the Nsangasi swamps stood, so to speak, at the end of the line. The Nsangasi lay at the southernmost limit of the floodplain of the Zaire, just before the river entered the narrow channel that cut through the high bluffs of the Bateke plateau. After Nsangasi, there were no more swamplands to settle. The first Nunu settlers in Nsangasi were undoubtedly familiar with the principles of fish-farming, but they had to adapt what they knew to the new species of fish and the idiosyncrasies of the currents and the annual floods. The Ngiri receives water only from the north, but the Zaire in this area receives water from both north and south of the equator (regions that have contrasting rainfall patterns), which results in a complex sequence of rising and falling water. It is impossible to say how long it took the Nunu to adapt and perfect their techniques, but, once they achieved high efficiency, it is unlikely that they changed further.

A man with the aid of his children or kinsmen constructed a dam or a pond that a son or nephew inherited. Other kinsmen had a right to participate in the emptying of ponds for a share of the fish, but only as junior partners who got smaller shares. Descendants of junior partners had even fewer rights or none at all. Fishing grounds were in theory passed down matrilineally from man to sister's son, but the histories of Nunu fishing grounds reveal a variety of inheritance patterns. The dam built by Bokota, for example, went to his sister's husband, then to her son, Botuka, and then to his son Nsamonie. In effect, it passed down matrilineally in one generation and patrilineally in the next. The dam constructed by Epatia, however, went to his son, Nkobonda, and then to his son, Loponda, a straight patrilineal succession. Because of the vicissitudes of inheritance, some people ended up with several dams or ponds. Mongemba was one of these lucky ones; he inherited three dams.⁶³ The first dam had passed from Bokonongo to his brother, Libwa, who left it to his maternal nephew, Molilu. Then it went to the husband of Molilu's sister, who left it to Mongemba, his son. Mongemba's second dam came to him from a man who left it to his daughter, who left it to her son, who left it to his son,

⁶³ Mongemba, personal interview, July 7, 1981.

Mongemba. Mongemba's third dam came to him as the result of several complicated changes of ownership, the details of which even he did not remember clearly.

While people like Mongemba amassed fishing rights, younger sons or nephews or the simply unfortunate got squeezed out. These people then migrated farther into the swamps in search of unclaimed land where they could construct dams or ponds of their own. Mobility had traditionally been part of fishing life, and, while the younger sons may have envied the older ones, they accepted the results of the system and moved on. Enzimba's family history, which compresses the experiences of several generations, provides a sense of this continual movement:

Our ancestors came from the village of Bokaa. They went to build at Liombo, then at Lilebu. They built six dams at Mecamenzel and Bibuka, and they dug six ponds. They went to Masaa and there they built six dams. They left Masaa and came to build three dams at Bokungu. After that they went to build at Mboma.⁶⁴

The problems began when, after successive generations of slow expansion, the emigrants reached the end of the swamps. There they built villages such as Minsange, which came to be inhabited by poor, jealous, and quarreling people whose condition became a manifestation of the effects of the lack of fishing grounds. In Nunu folklore there is a special genre of apocryphal stories about Minsange.⁶⁵ One recounts the tale of a man who could not mourn for his dead friend because he coveted the dead man's fish; another tells the story of a man who mourned the loss of a fish more than the loss of his wife; and yet another is the tale of a man who married a woman in an attempt to usurp the lands of her parents. Taken together, these stories indicate that the Malthusian vise was starting to squeeze and that at least one segment of the swamps was in a state of crisis.

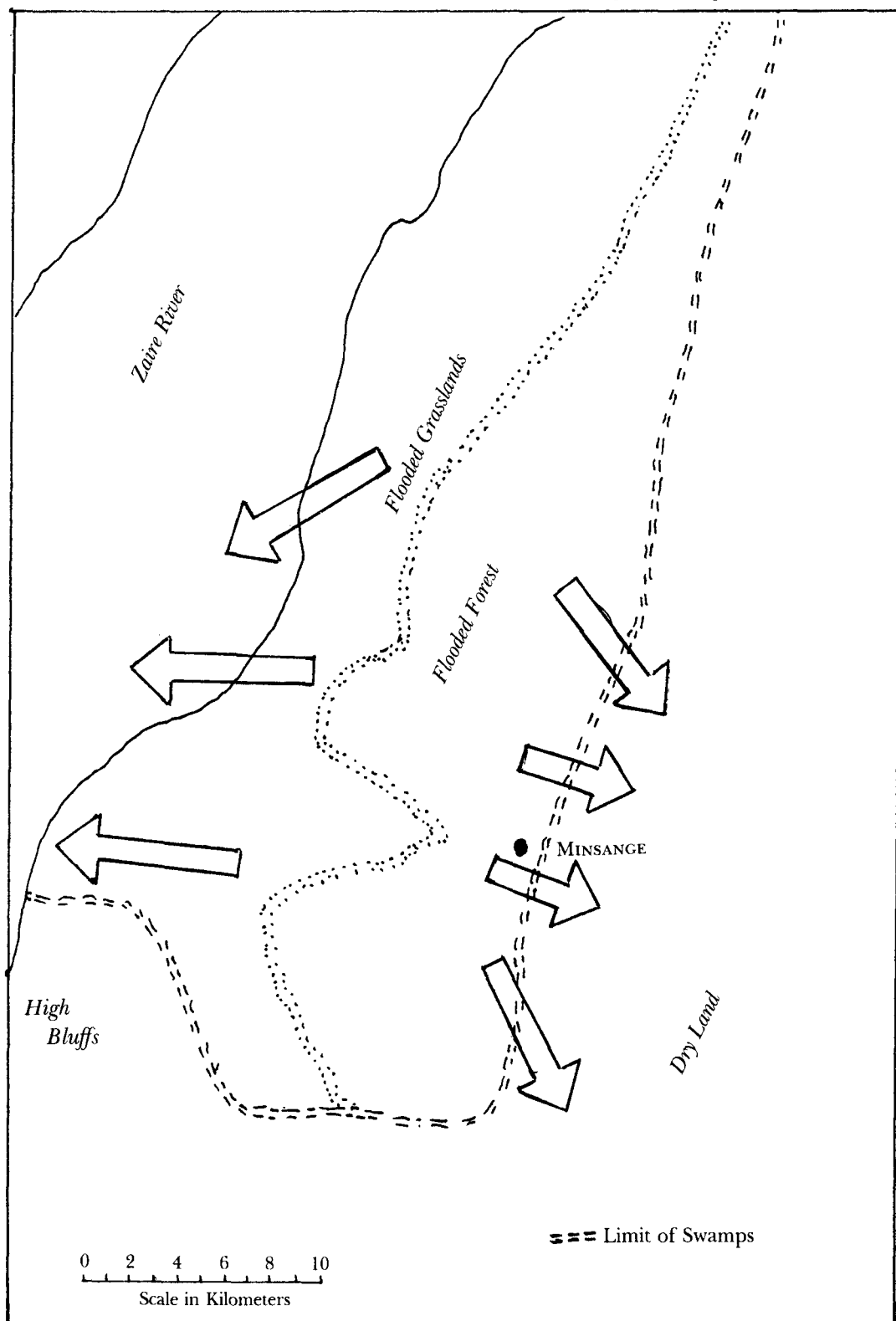
With fishing rights exclusively in the hands of the landholding class, the poor had three choices, none of which was desirable. One was to leave the swamps and become a farmer on the solid ground to the east. This meant not only a drastic change in a person's way of life but also a disadvantageous position in the marketplace. Farmers traded their cassava for fish from the swamps. Cassava, which was mostly starch, was considered a product of low value, and fish, a source of protein, had substantially higher relative value; therefore, it took a large amount of cassava to obtain a small amount of fish. The second option was to emigrate west to the Zaire River and migrate downstream to the towns built on the high bluffs along the channel. There the Nunu worked as river fishermen with lines, nets, and traps, all of which depended on the caprice of the fish. River fishing was not nearly as lucrative as fish-farming in the swamps. The third option was to stay in the swamps and become a client, working the dam or the pond of a rich man in return for a percentage of the catch. Client rights, like ownership rights, were passed from generation to generation. Yasi, for example, was the fourth generation to work as a client in charge of a certain dam.⁶⁶ Those who chose the third option formed a potentially explosive under-class; they sometimes conspired to seize dams or ponds, kidnapped women, and became involved in feuds.

⁶⁴ Mokengele Enzimba, manuscript family history, n.d.

⁶⁵ Lila, personal interview, July 14, 1981.

⁶⁶ Field notes, December 1975. Many of the emigrants were probably people who lacked even client rights.

MAP 2: Outward Migration from the Nsangasi Swamps



There is some indication that, at least for the eastern edge of the swamps, the wars of August became institutionalized sometime after the founding of Minsange. The evidence is that, in the late nineteenth century, people in villages that claimed ancestry in Minsange never attacked other villages claiming ancestry in Minsange. Instead, they fought those whom they called the people of Momamba, who had migrated to the eastern edge of the region via Masaa. If the wars had become institutionalized before Minsange was settled, ancestry in this village would not have served as a way of distinguishing one military alliance from another. And, if the wars had started long after emigrants from Minsange had established new identities to the east, the Minsange connection would not have been important. The process by which the wars became an institution is even less certain than the timing. Perhaps there were provocations that required revenge, which in turn required counterrevenge, and so on. Whatever the original reason, oral traditions, war dances, and war songs provide no clues. This omission is in itself interesting, and it confirms the hypothesis that the fighting was the goal of the wars. The most important question, however, is why, once the wars got started, they persisted until the early colonial period. The answer is probably that they served the interests of both the landed and the landless classes: the poor saw in them a chance for glory and immortality, and the rich saw in them a partial solution to a threatening social problem. The result was that the warrior tradition became enshrined in songs and dances and the rituals of war became more prominent and elaborate than any other rituals of the Nunu.

THE DIAGONAL NARRATIVE OF NUNU HISTORY avoids some of the problems of the horizontal slice and the vertical narrative, but it raises others. First, the narrative is about abstractions: change itself is the main character, and institutions are supporting actors. Yet institutions are created by people, and institutions affect life and death. By understanding changing institutions, it becomes possible to make inferences about the lives of people whose names are not known and whose individual struggles and triumphs have long been forgotten. If abstractions are often used to obscure the doings of individuals, they can also be used to bring to life the nameless people of a forgotten past. Another problem is that a single chain of causality has been created, a chain of causality that seems to ignore the possibility that a cause can have several effects and that an effect can have several functions. All of this simply reminds us that the history of the Nunu, like that of all peoples, contains more than one narrative, and that narratives can be greatly elaborated by the addition of subplots. For the sake of clarity, the narrative developed here has followed a single causal chain; but there are other chains that can be followed and other subplots that can be elaborated.⁶⁷

Many of the processes and structures outlined here run counter to the received notions about the social and economic history of the forest region. In the first place,

⁶⁷ Lawrence Stone has defined narrative as the "organization of material into a chronologically sequential order and the focusing of the content into a single, coherent story, albeit with subplots"; Stone, "The Revival of the Narrative: Reflections on a New Old History," *Past & Present*, no. 85 (1979): 3. I will elaborate on this theme in a book-length history of the Nunu that I am currently preparing.

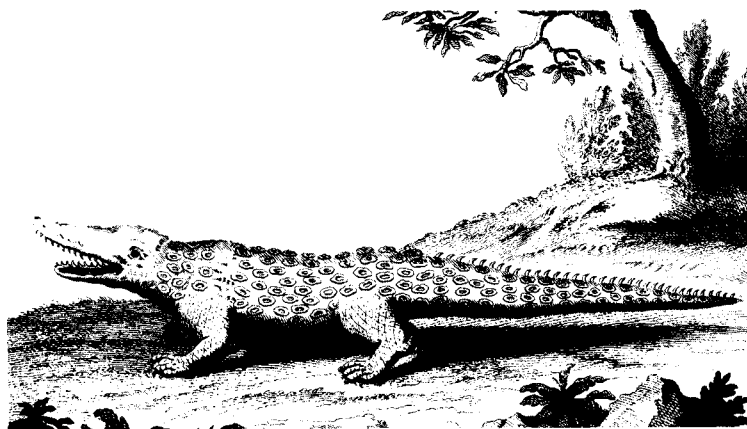
scholars have long viewed equatorial African societies as organized in terms of tribe, clan, and lineage, with kinship serving as the basis for social organization and with lineages forming the framework for the exchange of goods and services. Some historians have extended this notion to the point of identifying a lineage mode of production that structured life in equatorial Africa.⁶⁸ The history of the Nunu shows that, although ethnic identity, political solidarity, and kinship networks existed, as they do in all societies, the most historically significant social groupings were defined in terms of divisions between landowners and the landless, between patrons and their clients, much as they were in most other parts of the preindustrial world.⁶⁹ In the second place, the forest, with its small-scale forms of political organization, has generally been considered a backward part of Africa. But, in terms of the technology of production, the highly efficient systems of production developed by the fishermen of the equatorial forest surely make this one of the more developed parts of Africa, and the elaborate systems of dams and ponds bear comparison with the agricultural irrigation systems said to characterize highly developed societies in other parts of the preindustrial world. Unlike fifteenth-century Languedoc, described by Le Roy Ladurie, where inefficient production was a major determinant of historical processes, the very efficiency of the Nunu system, which allowed a few people to control the resources of the Nsangasi swamps, was crucial to Nunu economic and social history.⁷⁰

The history of the Nunu shows that the history of peoples of the equatorial forest cannot be explained by general theories of "primitive" behavior any more than can the history of Languedoc be explained in terms of changeless relationships between peasants and the land. The history of the fishermen of the Nsangasi, like that of the peasants of Languedoc, was a history of constant change and alteration as a result of the complex interaction of social, demographic, economic, and other factors. Only when all of them have been thoroughly explored will a history of the people of the equatorial forest emerge from the heart of darkness.

⁶⁸ Rey, *Colonialisme, Néo-Colonialisme*, 1–215.

⁶⁹ Harms, "Lineage Mode of Production"; and Vansina, "Lignage, idéologie, et histoire," 133–52.

⁷⁰ Le Roy Ladurie, *The Peasants of Languedoc*, 296.



Contours of Slavery and Social Change in Africa

PATRICK MANNING

THE PAST GENERATION OF AFRICANIST SCHOLARSHIP, with its profound revision in the interpretation of African social change, has raised questions about previous notions that change in precolonial Africa stemmed primarily from the influence of Europeans and other outsiders. According to the old view—the vision of “eternal Africa”—societies in tropical Africa changed at no more than a glacial pace, except under the relentlessly growing pressure of European intrusion. That this vision of eternal Africa survived until relatively recent times, if only among scholars whose work centered on other regions, is illustrated by H. R. Trevor-Roper’s famous characterization of African history as the study of “the unrewarding gyrations of barbarous tribes in picturesque but irrelevant corners of the globe.” A remarkable corpus of recent research has demonstrated, in contrast, the dynamic and innovative characteristics of precolonial African societies, particularly in the political, economic, and religious arenas.¹ As a result of this new literature, the Atlantic slave trade—that major avenue of contact between Africans and Europeans from the mid-fifteenth through the late nineteenth centuries—must now be viewed in a substantially different context.

The present study focuses on the impact of the Atlantic slave trade on precolonial African societies. In more general terms, it reconsiders the slave trade in terms of the relative importance of domestic and external sources of African social change. In addition to summarizing research on slavery and the slave trade, I hope to show, on the one hand, how the canons of African historiography have conditioned interpretations of the role of the trade in slaves and to indicate, on the

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¹ Trevor-Roper, “The Rise of Christian Europe,” *The Listener*, 70 (1963): 871. Historians who have followed Hugh Trevor-Roper’s recent efforts at authenticating documents will find his BBC television lecture—with its critique of journalistic fashion, of history based on speeches and battles, and of history beyond Europe—rewarding reading. Exemplary studies in the Africanist tradition include, in politics, Ivor Wilks, *Asante in the Nineteenth Century* (Cambridge, 1975); in economic history, Philip D. Curtin, *Economic Change in Precolonial Africa: Senegambia in the Era of the Slave Trade*, 2 vols. (Madison, Wisc., 1975); and, in religion, T. O. Ranger, *Revolt in Southern Rhodesia, 1896–7* (London, 1967).

other, what the implications of recent slave-trade studies are for broader interpretations of African history. The key elements unifying these empirical and interpretive considerations are a set of demographic and economic factors—changing prices, quantities, age and sex compositions of slave exports—and their links to African social structure.

THE OLD INTERPRETATIONS AND THE OLD DISPUTES on Africa and the slave trade have left their mark. The vision of eternal Africa allowed room for disagreement about the impact of the slave trade, and these disagreements have found their way into the current literature.² The contrasting interpretations separated those who contended that the transatlantic commerce in slaves brought major changes to African societies from those who denied that trade in slaves disturbed the African social order. Archibald Dalzel, the eighteenth-century English slave trader and propagandist, argued that African society remained unaffected, and he quoted King Kpengla of Danhomè in support of his position.³ David Livingstone, the mid-nineteenth-century explorer and missionary, argued forcefully that, to the contrary, slavery and the slave trade were devastating to African society.⁴ At the turn of the twentieth century Sir Harry Hamilton Johnston, an imperial man-on-the-spot and amateur scholar, attempted to synthesize these conflicting positions in an interpretation that is in some ways more optimistic than either: “Abominable as the slave trade has been in filling Tropical Africa with incessant warfare and rapine . . . , its ravages will soon be repaired by a few decades of peace and security during which this prolific, unextinguishable negro race will rapidly increase its numbers.”⁵

Both the similarities and the differences among these arguments are instructive. Dalzel and Johnston saw African societies as robust and able to withstand the pressures of the slave trade, while Livingstone viewed African societies as fragile and easily shattered. All three, however, shared the view of African societies as static. Although it had survived the slave trade intact, the “prolific, unextinguishable negro race,” in Johnston’s words, had not progressed—had not gained the ability “to start the children from a higher level than the parents.”⁶ This vision of eternal Africa, emphasizing stagnation and resistance to change, took root and

² Although pre–World War II studies on Africa commonly took a disparaging view of African society, some of them are very valuable sources for the history of African slavery and the slave trade. See, for example, Gustav Nachtigal, *Sahara and Sudan*, trans. Allan G. B. Fisher and Humphrey J. Fisher, 3 vols. (London, 1975); and Captain R. S. Rattray, *Ashanti Law and Constitution* (Oxford, 1929), 33–46.

³ Dalzel, *The History of Dahomy, an Inland Kingdom of Africa* (1793; reprint edn., London, 1967), 217–21. Also see Loren K. Waldman, “An Unnoticed Aspect of Archibald Dalzel’s *A History of Dahomey*,” *Journal of African History*, 6 (1965): 185–92; and I. A. Akinjogbin, “Archibald Dalzel: Slave Trader and Historian of Dahomey,” *Journal of African History*, 7 (1966): 67–78.

⁴ David Livingstone and Charles Livingstone, *Narrative of an Expedition to the Zambesi and Its Tributaries* (New York, 1866), 481–98; and David Livingstone, *The Last Journals of David Livingstone*, ed. Horace Waller, 1 (Hartford, Conn., 1874): 62–65, 97–98.

⁵ Johnston, *A History of the Colonization of Africa by Alien Races* (2d edn., Cambridge, 1913), 161–62; also see *ibid.*, 91. This volume, revised from the first edition of 1898, appeared in the Cambridge Historical Series and stood, for the first half of the twentieth century, as the most authoritative general text on African history.

⁶ Johnston, *History of the Colonization of Africa*, 162.

survived in the minds of observers largely because of the difficulty of knowing what changes had taken place. This difficulty, in turn, stemmed not only from the scarcity and dispersal of documentation on precolonial Africa but also from the blurring of perception brought about by cultural differences between African and European observers.

Contemporary Africanist historians have shown remarkable success in bridging the gaps within the documentary record and between cultures, not least because of the growing number of African contributors to the literature. Yet the rise of the Africanist tradition has not been sufficient to resolve the role of the slave trade in precolonial Africa. Instead, the earlier contending views—whether trade in slaves exerted great or small influence on African historical development—have aligned themselves with two interpretive tendencies that have grown up within the Africanist literature. For instance, Basil Davidson, writing in the early 1960s when many African countries were regaining their independence, argued, “Viewed as a factor in African history, the precolonial connection with Europe—essentially, the slave trade—had powerfully degrading consequences for the structure of society.” Some years later John D. Fage brought to the debate the revised figures on the magnitude of the slave trade; he concluded that the eighteenth-century loss of four million persons from West Africa did not reduce its population, and he later added that the region’s social institutions similarly remained unaffected.⁷ Considering many of the same data but also taking underdevelopment into account, Walter Rodney reasoned that, on the contrary, the trade in slaves had brought great harm to African economic and political structures.⁸ A decade later Joseph C. Miller argued, with reference to West Central Africa, that a domestically generated cycle of drought, disease, and famine did more to limit population and provoke social change than did the impact of slave exports.⁹

All of the participants in this recent discussion of the impact of the slave trade on Africa have assumed an African dynamism. The differences among them are in the relative emphases they have given to external forces of change. Can the external forces be safely minimized and treated as boundary conditions for a situation in which the major forces for change were domestic, as Fage and Miller have argued? Such an analytical approach may be termed the vision of “emergent Africa”: as the historical reconstruction of African social change has become increasingly detailed, this approach has become widely influential in the thinking of Africanist historians. Or must external factors be drawn fully into the analysis of precolonial forces for change, as Davidson and Rodney have contended? This is the vision of “Afrique

⁷ Davidson, *Black Mother: The Years of the African Slave Trade* (Boston, 1961), 277; and Fage, “Slavery and the Slave Trade in the Context of West African History,” *Journal of African History*, 10 (1969): 393–404, “The Effect of the Export Slave Trade on African Populations,” in R. P. Moss and R. J. A. Rathbone, eds., *The Population Factor in African Studies* (London, 1975), 15, and “Slaves and Society in Western Africa, c. 1445–c. 1700,” *Journal of African History*, 21 (1980): 289–310.

⁸ Rodney, *How Europe Underdeveloped Africa* (London, 1972), 103–11. Also see Rodney, “African Slavery and Other Forms of Social Oppression in the Upper Guinea Coast in the Context of the Atlantic Slave Trade,” *Journal of African History*, 7 (1966): 431–44.

⁹ Miller, “The Significance of Drought, Disease, and Famine in the Agriculturally Marginal Zones of West-Central Africa,” *Journal of African History*, 23 (1982): 30.

engagée,” which focuses on such interactions as much as on domestic evolution. The choice between the two is determined by ascertaining which leads to the most detailed and yet elegant interpretation of the historical record.

John Fage’s view of the role of the slave trade in precolonial Africa dominated the opinions of historians during the 1970s. For West Africa, Fage compared eighteenth-century slave exports that averaged forty thousand per year with a population he estimated at twenty-five million and a growth rate he estimated at 1.5 per thousand—or, some thirty-eight thousand—per year. Hence, “the effect of the export slave trade in the eighteenth century may have been more or less to check population growth,” and its impact was in consequence minimal even at the height of slave exports. Fage continued, “The conclusion to which one is led, therefore, is that whereas in East and Central Africa the slave trade, sometimes conducted in the interior by raiding and warring strangers, could be extremely destructive of economic, political and social life, in West Africa it was part of a sustained process of economic and political development.”¹⁰ West Africa’s domestic processes of evolution were potent enough, in Fage’s view, to absorb, neutralize, and, conceivably, even benefit from the effects of participation in slave exports. Although he admitted that the negative effects of slave trade might have been greater outside of West Africa, other scholars have claimed that, particularly for Central Africa, the population and social institutions successfully withstood the pressures of the slave trade.¹¹ This is the vision of emergent Africa, as applied to the impact of the trade in slaves.

Basil Davidson and Walter Rodney, while basing their views on the assumption of an evolving, developing African society, nevertheless asserted that the slave trade had a significantly detrimental effect on African society. Theirs was a vision of a precolonial Afrique engagée and of a continent that suffered from the engagement. The details of their arguments, however, rely not so much on demographic reasoning (both assumed that the African population did not decline) as on the interruption of African institutional and social progress. “The years of trial,” as Davidson called the precolonial era, “were years of isolation and paralysis wherever the trade with Europe, essentially a trade in slaves, could plant its sterilizing hand.” Although Davidson, with his slaves-for-guns thesis, and Rodney, with his underdevelopment thesis, listed striking examples to support this view, neither was able to develop a sustained and detailed analysis.¹² Moreover, their image of the meeting of European and African influences depicted not so much a true interaction of the two as the truncation of the latter by the former. Their interpretation thus veers

¹⁰ Fage, “Slavery and the Slave Trade in the Context of West African History,” 400.

¹¹ Roger Anstey, *The Atlantic Slave Trade and British Abolition, 1760–1810* (Cambridge, 1975), 79–82. John Thornton, in an analysis of the 1777–78 Portuguese censuses of Angola, emphasized the ability of the Angolan population to reconstitute itself even with the loss of many slaves; in a subsequent study, however, he concluded that the region’s population fell at the height of slave exports. Thornton, “The Slave Trade in Eighteenth-Century Angola: Effects on Demographic Structures,” *Canadian Journal of African Studies*, 14 (1980): 421–22, and “The Demographic Effect of the Slave Trade on Western Africa, 1500–1800,” in Christopher Fyfe and David McMaster, eds., *African Historical Demography*, 2 (Edinburgh, 1981): 691–720.

¹² Davidson, *Black Mother*, 273. Rodney did argue that African political development continued after 1500, but he failed to reconcile that position with his underdevelopment thesis. Compare his *How Europe Underdeveloped Africa*, 103–23, with *ibid.*, 124–48. For a critique of the slaves-for-guns theory of African coastal states, see Patrick Manning, “Slaves, Palm Oil, and Political Power on the West African Coast,” *African Historical Studies*, 2 (1969): 279–88.

back toward Harry Johnston's image of eternal Africa, except that Davidson and Rodney emphasized the negative, rather than the positive, contributions of European contact to African development.

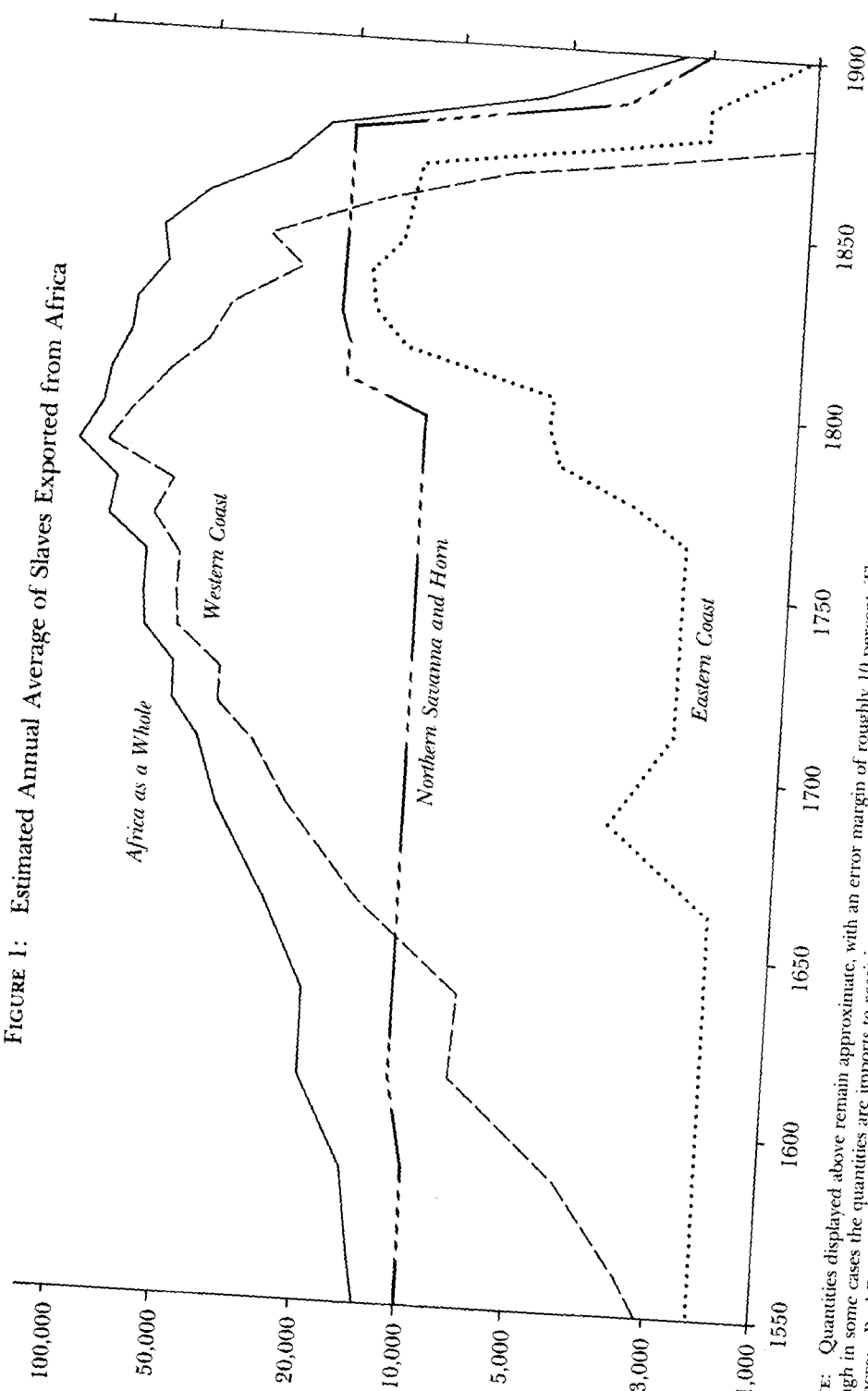
With a reformulation of the argument, however, the vision of *Afrique engagée* has gained validity as the relevant framework for interpreting the role of the slave trade in precolonial African history. The limits of Fage's interpretation are centered in his aggregative approach: he did not give much attention to the breakdown of slave exports by sex or age or to the impact of changes in quantities and prices of slave exports over time. Disaggregation of the data, when combined with analysis based on demographic principles and price theory, leads to results that are different from, and in some cases contradictory to, those previously accepted. The interactions of the New World demand for slaves with domestic conditions in Africa brought about—long before the late nineteenth century Scramble for Africa—pervasive social change. Such social changes included the expansion and subsequent transformation of polygyny, the development of two different types of African slavery, the creation and subsequent impoverishment of a class of African merchants, and a final expansion of slavery in the decades before the Scramble. Although the most profound changes from the interaction of the slave trade and African conditions occurred along the western coast of Africa, almost all regions of Africa were touched by the influence of the export trade at one time or another.

RECENT RESEARCH HAS CLARIFIED, to a remarkable degree, the dimensions of the export trade in African slaves. In addition to Philip Curtin's well-known estimation of the volume of the transatlantic slave trade, Ralph A. Austen has more recently estimated the volume of the slave trade across the Sahara, the Red Sea, and the Indian Ocean.¹³ The results of this research, updated to account for most subsequent revisions, are presented in Figure 1. Here the volume of exports is shown as estimated average annual exports for three great African regions: the western coast (from Senegal through Angola), the northern savanna and the Horn (from Senegal through Somalia), and the eastern coast (from Kenya through Mozambique and Madagascar).¹⁴ In age and sex composition, the populations of slave exports heavily favored young adults, those who were most valuable and best able to survive the ordeal of enslavement. Children, normally almost half the population in societies of that day, were quite underrepresented among slave exports. In the Atlantic trade, the preponderance of slaves transported was male, in a ratio of nearly two to one over women. This sex ratio varied according to, among other factors, the region of the coast from which slaves were shipped: men from

¹³ Austen, "The Islamic Red Sea Slave Trade: An Effort at Quantification," *Proceedings of the Fifth International Conference on Ethiopian Studies* (Chicago, 1979), 443–67, and "The Trans-Saharan Slave Trade: A Tentative Census," in Henry A. Gemery and Jan S. Hogendorn, eds., *The Uncommon Market: Essays in the Economic History of the Atlantic Slave Trade* (New York, 1979), 27–76; and Curtin, *The Atlantic Slave Trade: A Census* (Madison, Wisc., 1969).

¹⁴ Of the remaining portions of the continent, South Africa and Namibia exported virtually no slaves, although some domestic enslavement and slave importing took place, and North Africa was a slave-importing region. For the slave-exporting regions, the intensity of enslavement varied greatly, and even some adjoining regions were quite dissimilar: Togo exported few slaves and the Gold Coast exported many; Gabon exported few slaves, and the Congo exported many.

FIGURE 1: Estimated Annual Average of Slaves Exported from Africa



NOTE: Quantities displayed above remain approximate, with an error margin of roughly 10 percent. The graph is intended to reflect quantities of export from Africa, though in some cases the quantities are imports to receiving areas, which are slightly lower figures.

SOURCES: Paul E. Lovejoy, "The Volume of the Atlantic Slave Trade: A Synthesis," *Journal of African History*, 23 (1980): 480-81, 485, 490; Philip D. Curtin, *The Atlantic Slave Trade: A Census* (Madison, Wisc., 1969); Ralph A. Austen, "The Islamic Red Sea Slave Trade: An Effort at Quantification," *Proceedings of the Fifth International Conference on Ethiopian Studies* (Chicago, 1979), 443-67, and "The Trans-Saharan Slave Trade: A Tentative Census," in Henry A. Gemery and Jan S. Hogendorn, eds., *The Uncommon Market: Essays in the Economic History of the Atlantic Slave Trade* (New York, 1979), 27-76; Edward A. Alpers, *Ivory and Slaves from the East Coast of Africa* (Berkeley and Los Angeles, 1975), 187, 213, 238; and Allen F. Isaacman, *Mozambique: The Africanization of a European Institution* (Madison, Wisc., 1972), 86, 92.

Angola, where the slaves came from the far interior, exceeded women by as much as three to one, while the sex ratio was more nearly equal in the Bight of Biafra and other regions where slaves were drawn from coastal areas.¹⁵ For slaves exported to the Middle East, the sex ratio was reversed: women exceeded men among those slaves exported.¹⁶ Prices of African slaves, however, are not as well known as the quantities and sex composition of slaves exported. The main studies to date suggest, however, the pattern displayed in Figure 2, which shows estimates of average purchase prices for slaves on the western coast of Africa (in constant £1913).¹⁷ These prices thus represent the real earnings of the African sellers of slaves. Within this overall pattern there was great local variation as well as sharp fluctuations in slave prices associated with famines, epidemics, and warfare.¹⁸ Prices for female slaves peaked somewhat later and fluctuated less than those for male slaves.

These patterns of volume and prices reflect the changing pressures of African, Middle Eastern, and, especially, New World demand. Domestic African purchasers of slaves valued women more highly than men, since the social productivity of women (not only as domestics and wives but also as laborers) exceeded the economic productivity of men (as agricultural or artisanal laborers). In North Africa and the Middle East, the relative demand for women was even greater than in sub-Saharan Africa, although male slaves were employed there in agriculture and military service. The highest priced slaves in the Middle East, however, were eunuchs, which serves to accentuate the basically female orientation of slavery in that region.¹⁹

New World demand, meanwhile, was primarily for male slaves, and prices for men there were higher than for women. For the years from 1650 to 1800, the high productivity of New World slave labor and high purchasing power of slave owners, particularly in the sugar industry, generated a level of demand that governed prices of slaves throughout the world. As the New World demand for slaves continued to grow in the eighteenth century, prices for both male and female slaves rose in Africa and in the Middle East, which tended to choke off demand for slaves in those areas. But in the late eighteenth century, the New World demand reached its peak and began a slow decline, for a variety of reasons: a slackening growth in sugar production, the growing campaign against the trade in slaves, and, later on, the rise of an alternative source of sugar in the European beet and the opening up of alternative sources of New World field labor. The successive steps in the abolition of the slave trade—first in the Atlantic and then in the Indian Ocean, North Africa,

¹⁵ Patrick Manning, "The Enslavement of Africans: A Demographic Model," *Canadian Journal of African Studies*, 15 (1981): 517.

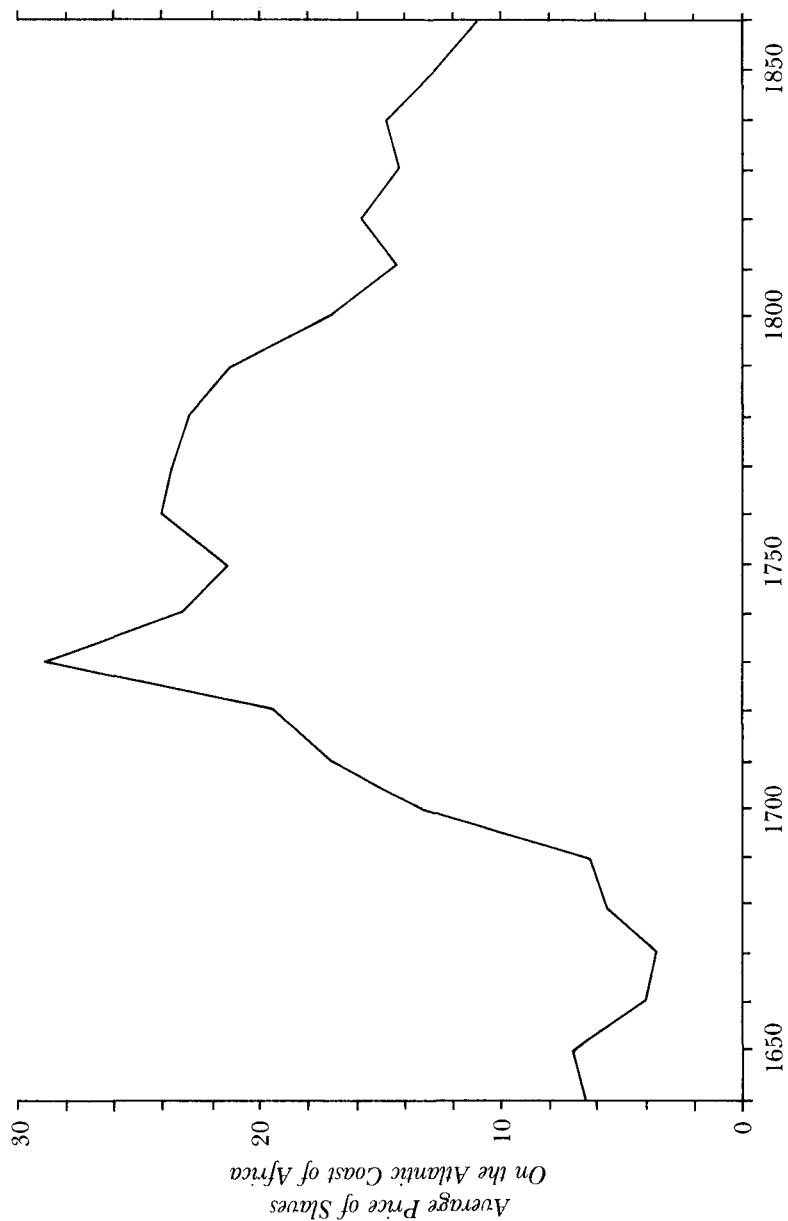
¹⁶ Austen, "The Trans-Saharan Slave Trade," 44.

¹⁷ Prices are given in £1913 because 1913 was the last year of a more than two-century period of overall price stability; the main fluctuation in prices during the eighteenth and nineteenth centuries was a price rise associated with the Napoleonic wars and a subsequent decline almost to prewar levels.

¹⁸ On the range of prices, see Richard N. Bean, *The British Trans-Atlantic Slave Trade, 1650–1775* (New York, 1975), 158–59; and Frederick Cooper, *Plantation Slavery on the East Coast of Africa* (New Haven, 1977), 123, 136–37.

¹⁹ Austen, "The Trans-Saharan Slave Trade," 44–46; and Roberta Ann Dunbar, "Slavery and the Evolution of Nineteenth-Century Damagaram," in Suzanne Miers and Igor Kopytoff, eds., *Slavery in Africa: Historical and Anthropological Perspectives* (Madison, Wisc., 1977), 161.

FIGURE 2: Prices of Slaves Exported from the Western Coast of Africa



NOTE: Prices are in 1913 £.

SOURCES: Richard N. Bean, *The British Trans-Atlantic Slave Trade, 1650-1775* (New York, 1975), 135-60; E. Phillip LeVeen, *British Slave Trade Suppression Policies, 1821-65* (New York, 1977), 8, 113; and Patrick Manning, *Slavery, Colonialism, and Economic Growth in Dahomey, 1640-1960* (Cambridge, 1982), 332-34.

and Asia—brought a decline in external demand for African slaves and a sharp drop in slave prices. From this point, domestic African, rather than external, demand governed slave prices.²⁰ Thus the expansion of nineteenth-century African slavery coincided with the decline in slave exports not only because more slaves remained in Africa but also because they became cheaper.

This continental perspective on the demography and economics of slave exports is supplemented and confirmed by the growing number of detailed local studies, whose contributions have included year-by-year totals of slave exports, specifications of which ethnic groups were exported, descriptions of the techniques of enslavement, analyses of the demography of the remaining population, and analyses of the conditions of slave production in Africa.²¹ The emergence of broad patterns has, further, prompted several scholars to propose general demographic models. In accounting for the size and composition of the export slave population, these models reveal by implication the size and composition of the remaining African population. The models generally focus on the western coast of Africa, although the rest of the continent has received some consideration. All tend to provide for a “catchment area” behind a given coast from which slaves were drawn, including a peripheral population that yielded slaves and a central population that both collected slaves and forwarded many of them to external markets.²² The models, which assume two stages of mortality (upon capture and upon export from Africa), have been used to argue that slave exporting reduced the population for those groups hardest hit by enslavement as well as for whole African regions and that the composition of the population was greatly distorted by the selective age and sex characteristics of exported slaves. Thus the models, used to organize data on the demography and prices of African slave exports, aid in the consideration of the impact of slavery and the slave trade on African society at each point in the course of the great expansion and eventual decline in the export demand for slaves. Their use represents an explicit adoption of the vision of *Afrique engagée*.

THE RECORDS OF THE FIRST PORTUGUESE RECONNAISSANCE along the African coast confirm that slavery existed in ancient Africa—as, indeed, in virtually all corners of the world. In addition to slavery, polygyny and monarchy were clearly developed in fifteenth-century African societies, notably those of Jolof, Benin, and Kongo.²³

²⁰ Claude Meillassoux, “Rôle de l’esclavage dans l’histoire de l’Afrique occidentale,” *Anthropologie et Sociétés*, 2 (1978): 130–31.

²¹ For year-by-year totals of slave exports, see David Birmingham, *Trade and Conflict in Angola* (Oxford, 1966), 137, 141, 154–55; and Joseph C. Miller, “Legal Portuguese Slaving from Angola: Some Preliminary Indications of Volume and Direction, 1760–1830,” *Revue française d’histoire d’Outre-Mer*, 62 (1975): 135–71. On identification of specific ethnic groups, see Patrick Manning, *Slavery, Colonialism, and Economic Growth in Dahomey, 1640–1960* (Cambridge, 1982), 335–43. On methods of enslavement, see David Northrup, *Trade without Rulers: Pre-Colonial Economic Development in South-Eastern Nigeria* (Oxford, 1978), 65–80. On the demography of the remaining population, see John Thornton, “The Slave Trade in Eighteenth-Century Angola,” 417–27. And, for an analysis of slavery in East Africa, see Cooper, *Plantation Slavery on the East Coast of Africa*, 153–212.

²² Manning, “The Enslavement of Africans,” 499–526; John Thornton, “Demographic Effect of the Slave Trade on Western Africa,” 691–720; and J. E. Inikori, Introduction to Inikori, ed., *Forced Migration: The Impact of the Export Slave Trade on African Societies* (London, 1982), 19–45.

²³ Curtin, *Economic Change in Precolonial Africa*, 1: 8–11; A. F. C. Ryder, *Benin and the Europeans, 1485–1897* (London, 1969); and Vansina, *Kingdoms of the Savanna* (Madison, Wisc., 1966), 38–46.

Before the New World came to dominate the African market for slaves, the Middle Eastern slave trade caused a slow but steady drain on the African population. Although the regional and temporal variations are difficult to reconstruct in detail, it appears that women crossed the desert to Morocco, Tripoli, Egypt, and South Arabia in greater numbers than did men, leaving more men than women in the African savanna. Those who became slaves within Africa were generally obtained in war; the victors got women for their harems, men for their fields, and additional slaves to export. In the African savanna, then, the losers lost population to the winners, who created a servile class, predominantly male and predominantly agricultural, from which a few slaves could be selected to rise as soldiers, domestics, or bureaucrats. So it was in Songhai, Borno, and Sinnar in the sixteenth and seventeenth centuries.²⁴ The constraints on the expansion of slavery in these states, however, corresponded to the limits on the power of the sovereigns and the limited market for slave-produced goods. The early growth of this savanna system of slavery and trade in slaves may, in turn, have served to reinforce the development of slavery in adjoining African areas even before the era of maritime contact with Europeans.

The Atlantic slave trade before 1650 rarely carried more than ten thousand slaves per year. Its aggregate impact on the continent was, therefore, small, although the experiences of certain regions prefigured the sharp pressures that were subsequently felt on a broader scale. The Kongo kingdom, for instance, became the main source of slaves for the sugar plantations of São Thomé in the sixteenth century, and numerous sources confirm the corrosive effect of slave exports on Kongo political structure and on the spread of Christianity there.²⁵ The Upper Guinea Coast supplied large numbers of slaves (predominantly male) as New World laborers in the early seventeenth century, and accounts of contemporary observers give strong support to the image of a predominantly female society left behind, in which women took over agricultural and fishing tasks to assure sufficient production.²⁶ Although only these two regions, Senegambia, and perhaps the kingdom of Benin exported enough slaves in these early days to influence population size and composition, virtually every region on the western coast of Africa provided some slaves to European purchasers in the years before 1650. But slaves had not yet become Africa's dominant export: gold exports, especially from the Gold Coast, exceeded the value of African slave exports until the end of the seventeenth century.²⁷

A qualitative change in the slave trade took form at the turn of the eighteenth century; a four-fold increase in slave prices occurred within thirty years. From the

²⁴ Meillassoux, "Rôle de l'esclavage dans l'histoire de l'Afrique occidentale," 119–28; J.-P. Olivier de Sardan, "Captifs ruraux et esclaves impériaux du Songhay," in Claude Meillassoux, ed., *L'Esclavage en Afrique précoloniale* (Paris, 1975), 111–13; and J. L. Spaulding, "Farmers, Herdsmen, and State in Sinnar," *Journal of African History*, 20 (1979): 339–45.

²⁵ Vansina, *Kingdoms of the Savanna*, 45–69.

²⁶ John Thornton, "Sexual Demography: The Impact of the Slave Trade on Family Structure," paper presented at the annual meeting of the Canadian Association of African Studies, held in Toronto, May 11–14, 1982.

²⁷ Richard Nelson Bean, "A Note on the Relative Importance of Slaves and Gold in West African Exports," *Journal of African History*, 15 (1974): 351–56. On slaves' ethnic origins, see Frederick Bowser, *The African Slave in Colonial Peru, 1524–1650* (Stanford, 1974), 40–41; and Curtin, *The Atlantic Slave Trade*, 113.

late sixteenth century to the mid-eighteenth century, the quantity of slaves shipped across the Atlantic grew at an average rate of 2 percent per year. Driving this growing demand was the sugar plantation system, as Brazil came to be joined by Barbados, Jamaica, Martinique, and other colonies. By 1650 the New World had displaced the Middle East as the dominant destination of African slaves. During the final years of price stability in the seventeenth century, African suppliers developed efficient methods for delivering more slaves with no increase in cost.²⁸ But, by the opening of the eighteenth century, the limits on the ability of Africans to provide cheap slaves had been reached, and prices rose dramatically. The cost of obtaining slaves rose as prospective captives learned to defend themselves better and as middlemen and toll collectors interposed themselves into the process of delivery. For these reasons as well as actual population decline, slaves became scarce relative to the level of demand. The price increases in this period led to the establishment of something resembling a world market for slave labor, in which New World demand and prices for slaves were so high that both slave prices and the quantity of slaves moved were affected not only along the western coast but in many regions of the African continent as well.²⁹

Two African regions bore the brunt of the expansion in exports at the turn of the eighteenth century: the Bight of Benin and the Gold Coast. Both of these regions experienced numerous wars among small states near the coast, from which captives were sold to the Europeans. For the Gold Coast, it meant the eclipse of gold as the main regional export for a time; for the Bight of Benin it brought entry into large-scale Atlantic trade. In both cases, the volume of slave exports rose within three decades to a level that reduced the region's population, after which slave exports declined slowly. Similar patterns of export increase to the limits of population tolerance, with a subsequent slow decline in exports, can be traced for other regions at other times. For Upper Guinea, the most substantial export of slaves occurred between 1600 and 1630. For the Senegambia, after a spurt of exports in the sixteenth century, exports rose again to a peak in the late seventeenth century. For the Bight of Biafra, slave exports shot up from the 1740s to the 1760s and remained at a high level through the 1820s. The Loango coast experienced a sharp increase in export volume in the years from 1720 to 1740 and another increase to a still higher volume from 1780 to 1800. Angola also experienced two great spurts in exports: one in the mid-seventeenth century, after which slave exports declined, and one during the 1720s to 1740s, after which exports remained at a high level into the nineteenth century.³⁰

Each of these sudden regional expansions in the slave trade caused—and reflected—changes in the methods and the morality of obtaining slaves. War,

²⁸ Bean, *Trans-Atlantic Slave Trade*, 69. For indications that prices at this time had fallen from an earlier level, see Ray A. Kea, *Settlements, Trade, and Politics in the Seventeenth-Century Gold Coast* (Baltimore, 1982), 198–200.

²⁹ Demonstrating the existence of a continental market in slaves necessitates demonstrating the movement of slaves at many points on the continent in response to changes in price. A related set of short-distance moves, however, suffices as well as long-distance displacements of slaves. For effects of this sort in Morocco, for instance, see Allan R. Meyers, "Class, Ethnicity, and Slavery: The Origins of the Moroccan 'Abid,'" *International Journal of African Historical Studies*, 10 (1977): 443–61.

³⁰ Curtin, *The Atlantic Slave Trade*; Phyllis M. Martin, *The External Trade of the Loango Coast, 1576–1870* (Oxford, 1972), 91; and Birmingham, *Trade and Conflict in Angola*, 137, 154–55.

judicial procedures, and kidnapping were the main processes by which slaves were obtained—war predominated in most of West Africa, kidnapping predominated in the Bight of Biafra, and judicial procedures played a leading role in Central Africa. For the Gold Coast, Ray Kea has documented changes in the technology and social organization of war that preceded the expansion of slave exports there. As war was transformed from the combat of elites, with minimal and defensive war aims, to combat based on musketry, on the *levée en masse*, and on objectives of territorial conquest, a seemingly endless stream of conflicts and captives was unleashed.³¹ A similar transformation accompanied the rise of slave exports from the Bight of Benin.

Were the wars provoked by the desire to sell slaves, or were the captives simply a by-product of wars fought for other reasons? Observers of the slave trade have debated the question inconclusively ever since the Atlantic trade began. Philip Curtin posed this choice with an eye to sustaining the vision of emergent Africa: he contrasted a political model and an economic model for enslavement in the Senegambia and concluded that the evidence best supported the political model.³² E. Phillip LeVeen offered the economist's response. The decision to export slaves captured according to the political model depended on the level of prices, and thus slave exporting, as opposed to capturing, fits the economic model.³³ The test of the issue is the responsiveness of slave export quantities to price changes. Even for small and well-defined areas, the elasticity of slave supply fluctuated sharply with the passage of time. Much of the apparent disorder, however, can be resolved by distinguishing periods when the system of supply was stable and positive price-responsiveness was clear (1730 to 1800 in the Bight of Benin) from periods when the ability of merchants to supply slaves was either improving sharply (1740 to 1780 in the Bight of Biafra) or declining (1690 to 1730 in the Bight of Benin).³⁴ These three examples can be set within the vision of *Afrique engagée*. In the Bight of Benin in the eighteenth century, domestic and external forces were locked in an equilibrium of sorts; in the Bight of Biafra in the mid-eighteenth century, domestic conditions changed more rapidly than can be explained by the influence of external forces alone; and in the Bight of Benin from the late seventeenth through the early eighteenth century external influences were the main source of African sociodemographic change.

As the demand for slaves continued to grow, opportunities for restrictive and monopolistic practices arose. Richard Nelson Bean, who has collected the best data on prices, joined with Robert Paul Thomas to argue that the Atlantic slave market was competitive and did not allow for monopoly profits, since European shippers could escape price gouging in one African port by going to another.³⁵ The contrary position is based on the argument that, since European shippers required a speedy turn-around and good relations with their African suppliers, they were tied to a

³¹ Kea, *Settlements, Trade, and Politics in the Seventeenth-Century Gold Coast*, 322–36.

³² Curtin, *Economic Change in Precolonial Africa*, 1: 157–68.

³³ LeVeen, "The African Slave Supply Response," *African Studies Review*, 18 (1974): 9–28.

³⁴ Manning, *Slavery, Colonialism, and Economic Growth in Dahomey*, 40–42; and Northrup, *Trade without Rulers*, 55.

³⁵ Richard Nelson Bean and Robert Paul Thomas, "The Fishers of Men: The Profits of the Slave Trade," *Journal of Economic History*, 34 (1974): 885–94.

single port. Even without monopoly profits, however, some African slave exporters may have collected high rates of profit through economic rent—that is, those merchants who were able to obtain slaves at unusually low cost still sold them at the prevailing market price. The African revenues from slave exports, which rose along with prices at the turn of the eighteenth century, were almost all turned into expenditures on imported goods: the value and volume of these imports thus increased dramatically at the same time that export prices rose.

The sudden increases in mercantile profit and in the volume of imported goods simultaneously began to restrict African population size. The technique of using New World inventories of slaves to project the ethnic origins of slave exports has established that, for the Bight of Benin, slaves came almost entirely from the Aja-speaking peoples in and around the kingdom of Danhomè, near the coast. The full demographic drain on the Bight of Benin was concentrated on this group, which experienced a loss estimated at over 3 percent of the population each year for over forty consecutive years. This loss was sufficient to reduce the Aja population substantially over the course of a century, both in absolute terms and in relation to the surrounding ethnic groups, notably the Yoruba.³⁶

The increase in New World slave prices influenced the slave trade in the northern savanna of Africa. The higher prices attracted male slaves from the interior regions, where demand for them was low, to the coast for sale. Female slaves tended not to be sent on the long march to the coast, since the high prices for female slaves in the African and Middle Eastern markets removed the incentive to sell them to Europeans.³⁷ But the higher New World prices for slaves also tended to raise the prices for slaves in the Middle East. The result was to choke off demand and to limit the volume of slave exports across the Sahara and the Red Sea.

The coastal exports of young adult slaves, twice as many men as women, tended to transform the structure of the population and the organization of society. A surplus of women developed, so that polygyny was reinforced, and work done by women in some places replaced that done by men. African traditions of family structure and division of labor, no matter how deeply instilled, could not but bend before these demographic pressures. The tradition of female agricultural labor in the matrilineal belt of Zaire and Angola may have been strengthened as an accommodation to the shortage of men for production.³⁸ Similarly, although the institution of polygyny preceded the slave trade, its extent was reinforced by the surplus of women. In addition, a surplus of women meant that men did not need to wait until their late twenties and thirties to marry their first and second wives. Indeed, the fear of enslavement may have encouraged men to marry at a young age.³⁹ In the savanna areas, despite the relative shortage of women caused by slave exports to the Middle East, polygyny remained widespread. But, in those areas with a shortage of women, polygyny was a privilege reserved to the old and the

³⁶ Manning, *Slavery, Colonialism, and Economic Growth in Dahomey*, 32–33, 335–43.

³⁷ Manning, "The Enslavement of Africans," 552.

³⁸ It should be noted, however, that women predominated in the agricultural labor force in areas of southern Africa from which few slaves were exported.

³⁹ That is, the situation might have been analogous to the tendency of men to marry early as they are being called off to war.

powerful, whereas, in areas with a surplus of women, polygyny was within the reach of most adult men.

Historical and anthropological studies of African slavery often include efforts to classify systems of slavery that generally use pairs of terms to distinguish two main types. Thus, "Islamic" slavery of the savanna is distinguished from "lineage" slavery of the coastal regions; "market-based" slavery of plantations is distinguished from "kin-based" slavery with its "absorptionist" character.⁴⁰ These historical and anthropological categories fit rather neatly, however, with the demographic distinctions between the western coast of Africa and the northern savanna: coastal slavery was primarily the enslavement of women, while savanna slaves were predominantly men. Thus "lineage," "kin-based," "absorptionist" slavery included women drawn into families, where they lived in the households of their owners, became wives or concubines, and whose children came to have rights within the family. "Islamic" and "market-based" slavery included men who were placed apart in slave villages, who produced for the market, often had no family at all, and thus clearly formed a separate and subordinate, servile class. This predominantly male slavery of the savanna had existed as long as the Saharan slave trade carried a surplus of women; the slaves produced for the palaces and served the local market. Though of relatively long standing, this system did not grow sharply until late in the eighteenth century. But the predominantly female slavery of the coast did grow rapidly, in region after region, along with the growth in the export slave trade: important changes in coastal family and institutional structure may thus be traced to the seventeenth and eighteenth centuries.⁴¹

The demand for slaves affected African polities by causing them not only to participate in slave exports but also to attempt to end it. The kingdom of Benin successfully withdrew from supplying slaves early in the sixteenth century. The Oyo Empire, though by reputation long tied to the slave trade, probably contributed only minimally to the export of slaves from Africa until late in the eighteenth century. And Boubacar Barry has interpreted the political events of the late seventeenth century in the Waalo kingdom of Senegal as an abortive attempt to withdraw from the slave trade.⁴² It is for the kingdom of Danhomè that the issue of attempted withdrawal from the slave trade has caused the most controversy. There is a certain plausibility to the notion that, in an area that was ravaged by trade in slaves and that experienced depopulation along with internecine wars, one state should attempt to conquer the whole region to end such conflicts and prevent social collapse. The question remains whether King Agaja's wars of the 1770s were an

⁴⁰ Martin Klein and Paul E. Lovejoy, "Slavery in West Africa," in Gemery and Hogendorn, *The Uncommon Market*, 203; Suzanne Miers and Igor Kopytoff, "African 'Slavery' as an Institution of Marginality," in Miers and Kopytoff, *Slavery in Africa*, 3–81; and James L. Watson, "Slavery as an Institution, Open and Closed Systems," in Watson, ed., *Asian and African Systems of Slavery* (Berkeley and Los Angeles, 1980), 1–15.

⁴¹ Meillassoux, "Rôle de l'esclavage dans l'histoire d'Afrique occidentale," 123–28; and Paul E. Lovejoy, "Plantations in the Economy of the Sokoto Caliphate," *Journal of African History*, 19 (1978): 349–51. For peoples of the Congo basin, according to Wyatt MacGaffey, "not only the degree of stratification but the matrilineal emphasis is a function of the influence of the Atlantic trade on a fundamentally bilateral system"; MacGaffey, "Lineage, Structure, Marriage, and the Family amongst the Central Bantu," *Journal of African History*, 24 (1983): 184.

⁴² Ryder, *Benin and the Europeans*; Robin Law, *The Oyo Empire, c. 1600–c. 1836* (Oxford, 1977), 151, 158, 176–77; Manning, *Slavery, Colonialism, and Economic Growth in Dahomey*, 31; and Barry, *Le Royaume du Waalo: Le Sénégal avant la conquête* (Paris, 1972), 142–59.

attempt to do so. But Danhomè's invasion and subjugation by the distant yet powerful Oyo made any such expanded Aja state impossible, and from 1730 to 1830 Danhomè faced a ring of weakened but by no means vanquished enemies; the internecine wars provided the New World with an inordinately large proportion of its slave laborers.⁴³

The case of Asante, to the west, represents a slightly different resolution of the same problem. Asante rose in the late seventeenth century to challenge Denkyira, the Gold Coast's dominant power, and eventually succeeded in incorporating virtually all the Twi-speaking peoples. As a result, the export of the people of Asante declined after about 1730, and the large number of slaves exported from the Gold Coast in succeeding years came increasingly from the interior Voltaic peoples.⁴⁴ In Oyo, only with the constitutional crisis and a series of factional disputes that began in the 1770s did that kingdom's export slave trade become significant, and the magnitude of slave exports grew with the decay of the state.⁴⁵ Thus, although the slave trade certainly affected politics, the nature of the trade's impact varied sharply from polity to polity; sometimes, as in the case of Oyo, politics influenced the trade in slaves more than the trade influenced internal affairs.

THE AGGREGATE DEMOGRAPHIC IMPACT OF THE SLAVE TRADE ON Africa reached a peak in the late eighteenth century, when slave exports averaged some one hundred thousand per year, and this high level of export continued into the early nineteenth century. During this period, the foci of enslavement tended to move from west to east and from coast to interior. For this period, then, it is most appropriate to assert that slave exports diminished the African population.

One method of assessing the population drain on individual ethnic groups entails, as mentioned above, making a New World inventory of slaves' ethnic origins.⁴⁶ A second approach focuses on whole regions, assessing the ability of regional populations to reproduce themselves in the face of the population drain resulting from slave exports. Roger Anstey, David Northrup, and, most effectively, John Thornton have used this procedure to good effect.⁴⁷ Thornton's results indicate a decline in the population of the whole Congo-Angola region during the eighteenth century, and his analysis further suggests that, during the height of the export trade, most of the regions of the West African coast could have withstood the pressure from the trade in slaves only with difficulty. A third approach, continental in scope, has been adopted by Joseph Inikori. His method focuses not so much on the actual reduction in population as on the difference between the actual population of Africa and the counterfactual population that might have existed in the absence of slave exports.⁴⁸

⁴³ David Henige and Marion Johnson, "Agaja and the Slave Trade: Another Look at the Evidence," *History in Africa*, 3 (1976): 57–68; and Manning, *Slavery, Colonialism, and Economic Growth in Dahomey*, 42.

⁴⁴ K. Y. Daaku, *Trade and Politics on the Gold Coast, 1600–1720* (Oxford, 1970).

⁴⁵ Law, *The Oyo Empire*, 265–66, 281–82; and Manning, *Slavery, Colonialism, and Economic Growth in Dahomey*, 45. For a contrary view, see Paul E. Lovejoy, *Transformations in Slavery: A History of Slavery in Africa* (Cambridge, 1983), 78–80.

⁴⁶ See note 36, above.

⁴⁷ Anstey, *The Atlantic Slave Trade and British Abolition*, 79–82; Northrup, *Trade without Rulers*, 81–82; and Thornton, "Demographic Effect of the Slave Trade on Western Africa," 709–14.

⁴⁸ Inikori, Introduction to *Forced Migration*.

Yet another approach involves the estimation of the impact of the slave trade through computer simulation. In this case, a model African population is postulated, divided into raiding and raided groups, and assigned a series of demographic and slave-trade rates: fertility, mortality, age-sex composition of the captured population, division of the captives between domestic and exported slaves, and so forth.⁴⁹ Preliminary projections of the model's results suggest that the slave trade caused losses that, if not devastating to the continent, were certainly severe. For the western coast, a region with an estimated population of twenty-five million in 1700, some six million slaves left in the course of the eighteenth century. The total number enslaved is projected at some twelve million, with four million retained in domestic slavery and over two million lost to death in the course of enslavement.⁵⁰ Under these conditions, the African population in 1800 was substantially less than it would have been in the absence of the trade in slaves. For the northern savanna and Horn, whose exports rarely exceeded twenty thousand per year, losses in slave exports were felt more acutely than the numbers alone suggest, because of the predominance of women exported. As a result, the northern savanna and the Horn were probably unable to experience any increase in population during the seventeenth and eighteenth centuries.

The demographic drain of the slave trade interacted with Africa's periodic droughts and famines. As the experience of the nineteenth century suggests, the incidence of drought and famine served at once to increase and to decrease slave exports. The onset of bad times caused the destitute to sell themselves or their children into slavery; but the decline in population resulting from famine tended to reduce the number of slaves.⁵¹ Stephen Baier and Paul E. Lovejoy have documented the great drought of the northern savanna in the mid-eighteenth century, which caused hardship, migration, and economic decline. Climatic recovery in the following decades led to economic growth, which was reflected both in the increase in slave exports and in the rise of the Sokoto Caliphate.⁵² Jill Dias and Joseph C. Miller have documented a cycle of drought, famine, and epidemic in Angola that severely limited population growth.⁵³ These Malthusian checks on population provide the primary evidence for revising downward Inikori's estimates of the counterfactual African population: Africa's population would surely have been substantially greater without the slave trade, but to know how much so requires a better knowledge of the effects of famine and disease than we now have. Miller may, however, have gone too far in arguing that the limits imposed by disease and

⁴⁹ Patrick Manning, "The Impact of Slave Exports upon Africa: A Demographic Simulation," paper presented at the annual meeting of the Canadian Association of African Studies, held in Toronto, May 11–14, 1982.

⁵⁰ Note that these figures are estimates of the total number of persons enslaved, and so forth, during the course of a century; the number of persons in domestic slavery at the end of the century would be a different, and perhaps smaller, figure.

⁵¹ Gerald W. Hartwig, "Demographic Considerations in East Africa during the Nineteenth Century," *International Journal of African Historical Studies*, 12 (1979): 658–72.

⁵² Lovejoy and Baier, "The Desert-Side Economy of the Central Sudan," *International Journal of African Historical Studies*, 8 (1975): 570–74.

⁵³ Jill R. Dias, "Famine and Disease in the History of Angola, c. 1830–1930," *Journal of African History*, 22 (1981): 360–62; and Miller, "The Significance of Drought, Disease, and Famine," 17–62.

drought were so great that these factors, rather than the slave trade, provided the fundamental limits on Angola's population.⁵⁴

The selective export of women from the coastal regions had its greatest impact in the late eighteenth century. The results of the simulation model suggests that, while the sex ratio among those populations that lost slaves remained roughly equal, the proportion of adult women to men rose substantially for the western coast as a whole: the estimated ratio of adult women to men rose to roughly six to five among the raiding populations.⁵⁵ But in those areas with the heaviest participation in slave exporting, the disparity in the sex ratio became greater. John Thornton's analysis of the Portuguese censuses for Angola indicates that the ratio of adult women to men was as much as two to one. On the one hand, this sex ratio shows how African societies could attempt to cope with an enormous drain on the population with losing their ability to reproduce; the people of Angola virtually became a livestock herd to be harvested. On the other, one woman was exported for every two to three men, and the loss of the women's reproductive potential made it all the more difficult for the population to maintain itself. The bulk of the agricultural labor fell on the women who remained in Angola, and the incidence of polygyny remained high.⁵⁶

By the end of the eighteenth century, the New World's demand for slaves persisted at a somewhat reduced rate. Only then did the quantity of slaves supplied by western Africa begin to decline—and for two reasons. First, the region just could not continue to supply slaves at the previous rate, and, second, the prices of slaves sold on the western coast declined (in real terms). The first of these factors sent European slave traders around the Cape of Good Hope to seek supplies in East Africa; the second allowed buyers in Africa and the Middle East to expand their purchases of slaves as prices fell into the range of their purchasing power.

The East African slave trade began its expansion in the late eighteenth century as a direct result of changes in the trade from western Africa. This trade, which rose and fell within a century, had two branches: one fed into European slave plantations, first in the New World and then on the islands of the Indian Ocean, and the other fed into the growing system of slavery in the Middle East, including the plantations of Zanzibar and the Kenya coast. The first called for exports of male slaves, but the second was predominantly female in its oceanic portion.⁵⁷

The increased African and Middle Eastern demand for slaves also led to an expansion of slavery and the trade in slaves in the northern savanna. A related rise in volume of slaves exported from northern Nigeria to the coast can be traced to the 1780s. We do not know whether the increased Nigerian exports resulted from wars on Curtin's political model, in response to rising religious fervor, or whether the higher demand for slaves resulted in more wars, which called forth religious

⁵⁴ Miller, "The Significance of Drought, Disease, and Famine," 30.

⁵⁵ Manning, "The Impact of Slave Exports upon Africa," 8.

⁵⁶ Thornton, "The Slave Trade in Eighteenth-Century Angola," 422–25; and Manning, "The Enslavement of Africans," 521–23.

⁵⁷ Ralph A. Austen, "From the Atlantic to the Indian Ocean: European Abolition, the African Slave Trade, and Asian Economic Structures," in David Eltis and James Walvin, eds., *The Abolition of the Atlantic Slave Trade* (Madison, Wisc., 1981), 117–40.

justification. The reforming zeal of Usuman dan Fodio can be seen, on the one hand, as having led to the creation of the Sokoto Caliphate, with the captives and the slave trade as a side effect, or, on the other, as the political reorganization of a society that was becoming increasingly dependent on slave labor for production.⁵⁸ Whatever the causal relations, the interconnections were clearly of importance.

Even after slave prices began to inch downward, the wealth garnered by slave merchants remained remarkable. In real terms, slave prices were four times greater in the eighteenth century than they had been in the seventeenth, and much of that increase must have reflected profit, inasmuch as the physical cost of delivering slaves cannot have increased to that extent in the course of a century. The cost of capturing a slave was always low, and the prices of slaves purchased near the point of capture, which Curtin aptly characterized as the price of selling stolen goods, were low at all times.⁵⁹ The remainder of the price was made up of the cost of transporting, feeding, and otherwise providing for slaves, and by the fees, commissions, and bribes that had to be paid either to hold or to move slaves. The need to feed and provide for slaves led to the growth in market demand for food along slave trade routes. The fees and profits provided the basis for the growth of a wealthy commercial and aristocratic class, which used its wealth not only to purchase imported goods but to buy its way into positions of influence in domestic society. As Paul Lovejoy pointed out about the eighteenth century, “rather than the state controlling the trade, the merchants dominated the government.”⁶⁰

Yet, even though the slave trade led to the rise of a wealthy and exclusive class, only a small portion of imported goods properly qualified as luxuries. The greatest value of imports was usually in textiles, produced first in India and then, with the rise of Manchester, in England. Luxury textiles in Africa were African-made; the imports qualified as cheap if colorful material, which reached a large number of consumers.⁶¹ The next most important import was money—cowrie shells from the Maldives, gold from Brazil, European silver coin, copper rings, brass manillas, and cloth currency. The export slave trade was thus closely tied to a substantial expansion of the money supply in several regions of Africa.⁶² And here, too, all segments of society, not just an elite, were linked to the slave trade. Iron, salt, and alcoholic beverages were other major imports, for European supplies of these commodities were cheaper than their African counterparts. Iron imports, for instance, served to constrict iron mining in Africa, although the imported iron did

⁵⁸ Lovejoy, “Plantations in the Economy of the Sokoto Caliphate,” 349–51; Manning, *Slavery, Colonialism, and Economic Growth in Dahomey*, 30, 45; and R. S. O’Fahey, “Slavery and the Slave Trade in Dar Fur,” *Journal of African History*, 14 (1973): 29–43.

⁵⁹ Philip D. Curtin, “The African Diaspora,” *Historical Reflections*, 6 (1979): 14–16.

⁶⁰ Lovejoy has limited this characterization, however, to market towns and seaports; see his *Transformations in Slavery*, 101; and Robert W. Harms, *River of Wealth, River of Sorrow: The Central Zaire Basin in the Era of the Slave and Ivory Trade* (New Haven, 1981), 30–39, 43–47.

⁶¹ Marion Johnson, “Technology, Competition, and African Crafts,” in Clive Dewey and A. G. Hopkins, eds., *The Imperial Impact: Studies in the Economic History of Africa and India* (London, 1978), 259–69.

⁶² On cowrie currency, see Jan S. Hogendorn and Marion Johnson, “A New Money Supply Series for West Africa in the Era of the Slave Trade: The Import of the Cowrie Shell from Europe,” in *Slavery and Abolition* (forthcoming), and *Shell Money of the Slave Trade* (forthcoming). On imports of gold, see Kea, *Settlements, Trade, and Politics in the Seventeenth-Century Gold Coast*; on cloth currency imports, see Curtin, *Economic Change in Precolonial Africa*, 1: 237–39; and, on copper currency and manillas, see Northrup, *Trade without Rulers*, 161–65.

help expand the blacksmith trade. Finally, imports of firearms—though not of the militarily decisive character they assumed in the nineteenth century—were sufficient to decide the outcome of several military engagements. Although firearms rarely exceeded a small portion of the value of imports, their importation did show a correlation with the volume of slave exports.⁶³

BY THE MID-NINETEENTH CENTURY, a dramatic reversal in the character of the Atlantic slave trade had taken place. Most New World areas had dropped out of the slave trade, and slave imports were illegal in the remaining areas of demand—Cuba and Brazil. As a result, although the prices of slaves in the New World rose because of the scarcity of new imports, the export demand for slaves at any given price on the African coast had fallen significantly, and the price of slaves in Africa fell almost as significantly. The relatively scarce price data available for the early nineteenth century are somewhat contradictory, so the precise timing of the price decline remains to be confirmed. But it is clear that, sometime between 1780 and 1850, the price of slaves on the African coast fell, in real terms, by one-half.⁶⁴ The mechanisms of slave supply remained in place, however, so that a glut on the slave market became evident. This nineteenth-century glut brought a pervasive change in the character of African slavery: as slaves, particularly male slaves, came within the purchasing power of African buyers, the scope of African slavery expanded greatly in the mid-nineteenth century, although the total number of people captured may not have changed greatly.

As more women remained in Africa, the number of births dramatically increased; and, as more men stayed, the previous drain on the adult male population ended, although the process of enslavement for the African market still implied a significant mortality. These changes resulted in rapid population growth.⁶⁵ In addition, the large number of cheap male slaves now made the situation on the coast much more like that in the savanna, where male slaves were used for agricultural labor. Thus the coastal areas now developed slave plantations that produced for palace populations, for the African market, and for export. The mid-nineteenth-century growth in the export of palm oil, coffee, and peanuts thus reflected not only the rise in European demand for these products but also a significant decline in the cost of production because of the fall in slave prices.⁶⁶

⁶³ J. E. Inikori, "The Import of Firearms into West Africa, 1750 to 1807: A Quantitative Analysis," *Journal of African History*, 18 (1977): 339–68.

⁶⁴ The prices shown in Figure 2 for the (see page 000, above) for the 1790s through the 1850s are based on E. Phillip LeVeen, *British Slave Trade Suppression Policies, 1821–65* (New York, 1977), 3, 113. These prices are there listed only in summary form and without sources, so that it appears that the work of documenting them will have to be duplicated. Philip Curtin's carefully calculated prices for the Senegambia show an increase to a peak in the 1830s, followed by a decline; Curtin, *Economic Change in Precolonial Africa*, 2: 51–53. Also see David Tambo, "The Sokoto Caliphate Slave Trade in the Nineteenth Century," *International Journal of African Historical Studies*, 9 (1976): 187–217; and Gwyn Campbell, "Madagascar and the Slave Trade, 1810–1895," *Journal of African History*, 22 (1981): 203–27.

⁶⁵ Thornton, "Demographic Effect of the Slave Trade on Western Africa," 426.

⁶⁶ A. J. H. Latham, *Old Calabar, 1600–1891* (Oxford, 1973), 91–96; and George E. Brooks, "Peanuts and Colonialism: Consequences of the Commercialization of Peanuts in West Africa, 1830–70," *Journal of African History*, 16 (1975): 49–50.

The growing population moved rapidly to an overall parity in the sex ratio. Wealthy and powerful men were still able to collect harems, but there no longer existed such a surplus of women that many men could expect to have second wives at an early age. Thus, the “kin-based” slavery of the coastal regions in the previous era tended to be replaced by a “market-based” slavery, in which men lived in isolated slave villages and lost any ability to rise in the social order and in which many women, also in isolated slave villages, became part of an oppressed class rather than retaining the ability to experience progressive integration into their masters’ families.⁶⁷

The decline in slave prices brought a substantial transfer of wealth in Africa. Previously, the wealth from slavery and the slave trade had gone to the merchants and fee collectors who gathered in the great difference between the initial cost of capturing slaves, which remained low, and the market price of slaves. The price of slaves fell once it was no longer supported by New World demand, but the supply continued to be large as long as the price exceeded the initial cost of capturing slaves: the mercantile profit margin was squeezed unmercifully by market forces. Some merchants simply lost their fortunes, as did the Bobangi merchants of the Congo River and Francisco Felix da Souza in the Bight of Benin.⁶⁸ Others were able to invest their fortunes in land and benefit from the direct exploitation, rather than the sale, of slaves. Thus, this period saw the growth of an active land market, as wealthy speculators sought through purchase and state action to gain control of prime agricultural land, and, through exploitation of slave labor, to become large-scale planters.⁶⁹ Both rapid population growth and disputes over land ownership, therefore, are phenomena that Europeans observed in the early colonial years, but these phenomena may be traced back beyond the European conquest of Africa to the end of the export trade in slaves.

The East African slave trade hit its peak in the mid-nineteenth century, and the movements of immense numbers of people brought political turmoil to the region and spurred the spread of disease; cholera and smallpox in particular followed the paths of the slave trade. East African merchant fortunes never grew to the size of those in the earlier West African trade, because of the lower level of prices, although the boom in ivory provided some compensation to merchants. Instead, slave plantations arose in East Africa, as elsewhere on the continent, serving both the domestic market and, especially in the production of cloves, the export market.⁷⁰

The European conquest of Africa enforced, almost immediately, the end of slave raiding and the trade in slaves. But no firm act of emancipation followed to liberate those enslaved: so protracted and incremental was the process that the active New World term “emancipation” is replaced in much of the African literature with the

⁶⁷ Latham, *Old Calabar, 1600–1891*, 94–96, 121.

⁶⁸ Harms, *River of Wealth, River of Sorrow*, 156–59; and J. Michael Turner, “Les Bresiliens: The Impact of Former Slaves upon Dahomey” (Ph.D. dissertation, Boston University, 1975), 88–98.

⁶⁹ A. G. Hopkins, “Economic Imperialism in West Africa: Lagos, 1880–92,” *Economic History Review*, 21 (1968): 580–606.

⁷⁰ Cooper, *Plantation Slavery on the East Coast of Africa*.

passive “decline of slavery.”⁷¹ With the conquest, many slaves, particularly males, ran off, and many others, particularly females, remained under their masters with renegotiated conditions of servitude. Colonial administrations, unwilling to compensate slave owners but equally unwilling to face the wrath of owners who had been forced to liberate slaves without compensation, generally tended to ensure that all children born after a given date would remain free.⁷² But, with few exceptions, the force of the European conquerors as well as the voluntarism of African slaves ended slavery in Africa rather than any action of leading figures within African society.

African slave owners, therefore, recognized the fundamental threat of the onset of European conquest: they stood to lose not only their sovereignty but also the full benefits of the economic system on which their strength was based. Thus it is that one more major African religious revolution, the Mahdist movement of the Nilotic Sudan, is susceptible to interpretation as an economic movement as well as one of faith. In 1877, the Khedive Isma'il, the Egyptian ruler of the Sudan, concluded an anti-slave-trade pact with Britain and appointed Major-General Charles George Gordon as governor of the Sudan. Those who then joined with the Mahdi to proclaim a rightly guided, theocratic state included slave merchants and planters, whose slave villages produced grains and manufacturers for domestic sale and export.⁷³ Their defeat of Gordon in 1885 granted them more than an additional decade in which to direct an economic system based heavily on slave labor.

Quite aside from the European pressures to dismantle the newly expanded African slave-labor mode of production, the African system of slavery tended to evolve in two—essentially contradictory—directions under the pressures resulting from the expansion of the scale of slavery. The continued enslavement of large numbers of people and the low prices of slaves were at the root of both developments. At one extreme, so many people had been placed in slave villages that the problem of social control became serious. In the Sokoto Caliphate and Zanzibar, slave owners realized that providing women for the slave men helped them maintain social control, and, in the Sokoto Caliphate, slavery tended to evolve toward serfdom and, therefore, toward a limit on the abuses and human destruction of the system.⁷⁴ At the other extreme, the declining value of slaves led them to be increasingly considered as expendable. In the military actions of the western savanna in the late nineteenth century, male captives, whose ability to resist made them a threat and whose market value was low, were sometimes simply executed. Similarly, the reported increase in the number of human sacrifices in the kingdoms

⁷¹ For an excellent case study that uses this terminology and also indicates why it has been adopted, see Richard Roberts and Martin A. Klein, “The Banamba Slave Exodus and the Decline of Slavery in the Western Sudan,” *Journal of African History*, 21 (1980): 375–94.

⁷² Patrick Manning, “Un Document sur la fin de l’esclavage au Dahomey,” *Notes africaines*, 147 (1975): 88–92; and Frederick D. Lugard, *The Dual Mandate in British Tropical Africa* (London, 1922), 365, 372, 375–76.

⁷³ Gabriel R. Warburg, “Ideological and Practical Considerations Regarding Slavery in the Mahdist State and the Anglo-Egyptian Sudan, 1881–1918,” in Paul E. Lovejoy, ed., *The Ideology of Slavery in Africa* (Beverly Hills, Calif., 1981), 248–57.

⁷⁴ Paul E. Lovejoy, “Characteristics of Plantations in Nineteenth-Century Sokoto Caliphate (Islamic West Africa),” *AHR*, 84 (1979): 1269, 1290–92; and Cooper, *Plantation Slavery on the East Coast of Africa*, 221–23.

of Danhomè and Benin can be related not only to the pressures of imminent European conquest but also to the declining value of slaves. The frequency of witchcraft accusations and attendant executions, moreover, also seems to have risen in this period, which provides another example of both the declining value of humans and the psychic pressures on societies facing conquest.⁷⁵

The European conquerors, in crushing the African slave-labor system, tended to bring about an equalization in African society. The slaves used their increased freedom to seek improved status, while those whose wealth had been based in slaves had to contrive to transfer that wealth into other resources or simply lose it. But the change in Africa's labor systems, while substantial, was by no means absolute. One case makes explicit the connection between precolonial and colonial systems of labor migration: the Gaza state in Mozambique captured and exported slaves to Madagascar and the Mascarenes from mid-century to the 1870s, and thereafter sent men who were similarly recruited as contract laborers for the mines in South Africa.⁷⁶ Of the many other parallels between precolonial and colonial labor systems, one more may suffice: the migration of Mossi laborers to work on the cocoa farms of the Gold Coast and Ivory Coast in the twentieth century closely resembles the earlier movement of Mossi slaves to the same area.

ETHNOGRAPHERS OF THE EARLY TWENTIETH CENTURY, writing in the last days of slavery, described the institution as relatively benign, emphasizing the legal and societal protections available to slaves as well as their potential for upward mobility.⁷⁷ These reports—written after slave raiding and the trade in slaves had ceased and after slaveholders had lost the support of the state—stand in sharp contrast to the travelers' reports of the late nineteenth century, which tell of brutal raids, immense loss of life, and massive exploitation of slaves by masters.⁷⁸ Each of these views of African slavery was appropriate to the precise time at which it was written. Both views, but particularly the former, have in turn been taken by subsequent scholars as appropriate characterizations of African slavery across the centuries.

The vision of emergent Africa, based on the assumed existence of continuous pressures for change within African societies, tends to suggest that both of these static views of servile institutions were invalid, without posing an alternative. The vision of *Afrique engagée*, by explicitly reintroducing external forces of causation into a framework that assumes an African dynamism not only confirms that

⁷⁵ Wyatt MacGaffey reported that "Monteiro, describing Angola in the 1860s, wrote that witchcraft accusations were the chief source of slaves for shipment, and argued that the reductions of the slave trade had caused the number of executions to rise." MacGaffey, "Economic and Social Dimensions of Kongo Slavery (Zaire)," in Miers and Kopytoff, *Slavery in Africa*, 254. Also see Meillassoux, "Rôle de l'esclavage dans l'histoire d'Afrique occidentale," 130; and Ryder, *Benin and the Europeans*, 247.

⁷⁶ Patrick Harries, "Slavery, Social Incorporation, and Surplus Extraction: The Nature of Free and Unfree Labour in South-East Africa," *Journal of African History*, 22 (1981): 317–20, 328–39.

⁷⁷ See, for example, Northcote W. Thomas, *Anthropological Report on the Ibo-Speaking Peoples of Nigeria*, 4 vols. (1913–14; New York, 1969), 1: 103–07, and 2: 111–25; and Rattray, *Ashanti Law and Constitution*, 33–46.

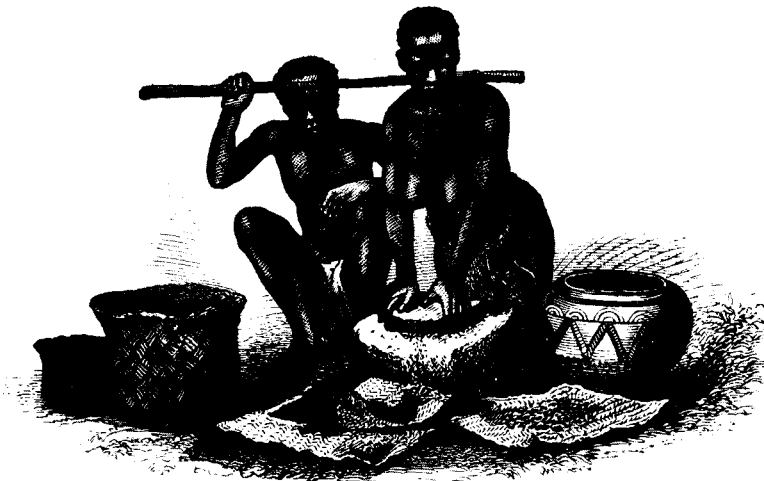
⁷⁸ The writings of David Livingstone were outstanding in this regard. Livingstone and Livingstone, *Expedition to the Zambesi*, 412–13, 481–98, 620–23; and Livingstone, *Last Journals* 1: 62–65, 88–89, 97–98.

suggestion but indicates the nature and timing of some important African social changes. In so doing, this historiographical approach must admit to a range in the type of interactions. In some cases, domestic forces dominated the interactions; the expansion and transformation of polygyny under the influence of the slave trade, for example, took place in the context of a previous African attachment to multiple marriage. In other cases, external forces dominated the interaction; both depopulation and the impact of imported goods, for instance, represent the domination of outside influences. And the precise combination of domestic and external forces provided the key impetus to certain changes, notably in the rise of Asante and Danhomè, the collapse of Kongo, and the mutual reinforcement of the slave trade, famine, and epidemic.

The return on this increased complexity in analytical framework is a clearer time-perspective on African society.⁷⁹ African slavery, along with a range of associated institutions, underwent successive transformations in the seventeenth, eighteenth, and nineteenth centuries under the impact of changing economic, demographic, and political conditions. Suzanne Miers and Igor Kopytoff, in an essay that sits firmly within the emergent Africa tradition of analysis, have gone so far as to criticize the use of the term "slavery" in Africa on the grounds that it implies a greater uniformity in African institutions of servitude than is warranted.⁸⁰ Their emphasis on the specificity, in sociological cross-section, of African systems of slavery is valid in principle if somewhat exaggerated in practice. To their sociological specificity must be added, however, the specificity of African societies in historical time-perspective, as they changed through the action of the diverse creative powers within them and the varying external pressures upon them. In the era of the slave trade, the external influences were so powerful as to set in motion comparable trends in social change in many parts of the African continent two centuries before the colonial conquest did in a vastly different fashion.

⁷⁹ Analogous approaches are being applied to other aspects of precolonial African history. On domestic and Islamic influences in state growth, for instance, see O'Fahey, *State and Society in Dar Fur*.

⁸⁰ Miers and Kopytoff, "African 'Slavery' as an Institution of Marginality," 3–7.



Laboratory for the Oral History of Slavery: The Island of Lamu on the Kenya Coast

PATRICIA ROMERO CURTIN

IN 1907, THE SULTAN OF ZANZIBAR, on orders from his supervisors in the British Foreign Office, outlawed the practice of slavery on the Kenya coast—an action that affected many aspects of coastal societies, especially for the slaves themselves.¹ Most slaves on the mainland quickly deserted their owners' plantations. Those who stayed farmed the land as squatters, refusing to labor for their former owners. But on the island of Lamu, and particularly in Lamu town emancipation changed little in the lives of the slaves. Control over island slaves had always been greater than over slaves on the mainland. And Islamic law was more strictly observed among both slaves and their owners on Lamu than elsewhere on the Kenya coast. In the eyes of the Lamu faithful, only masters had the right to free their slaves; here the secular proclamation issued by the sultan and brought to the island by British agents had little effect.

There is no way of knowing how many Lamu slaves chose freedom, when British commissioners arrived to compensate their owners and to inform the slaves of their rights under the law. Most who did went elsewhere to seek employment, an option unknown, perhaps, to some who stayed. Perhaps half of the slave population—including whole families—remained under a system of *de facto*, if not *de jure*, slavery. As Lamu fortunes declined, owing to the loss of mainland revenue, slaves abandoned masters who could no longer support them. But in form and under Islamic law the institution of slavery continued in some measure, and among some families, until Kenya achieved independence in 1963–64. Slavery was continued by collusion among the old Afro-Arab families² (including relatives of the sultan³),

I wish to thank Professors Frederick Cooper, Philip Curtin, William Freehling, and Sidney Mintz for their generous criticisms and suggestions in revising earlier drafts of this essay.

¹ For a thorough and very original account of slavery on the East African mainland, see Frederick Cooper, *Plantation Slavery on the East Coast of Africa* (New Haven, 1977). Also see Marguerite Ylvisaker, *Lamu in the Nineteenth Century: Trade and Politics* (Boston, 1979). Ylvisaker's work encompasses relations with neighboring islands and includes some coverage of slavery on the mainland. For coverage of exslaves on the Kenya coast, including those who left Lamu, see Frederick Cooper, *From Slaves to Squatters: Plantation Labor and Agriculture in Zanzibar and Coastal Kenya, 1890–1925* (New Haven, 1980).

² Swahili-speakers on the coast of Kenya—those whose first language is Swahili—today refer to themselves as Swahili in many circumstances, but ethnic designations from the recent past are complicated by political circumstances. During the colonial period, the ruling class of Afro-Arabs referred to themselves as Arabs to distinguish themselves from their slaves and from other Africans on the mainland. Others, however, used the

despite successive political regimes and authorities (the Sultanate, East African Protectorate, and Kenya Colony).

Masters' treatment of slaves and slave attitudes toward masters naturally altered with the passage of time. And yet data collected in interviews conducted in the early 1980s during my field work in Lamu—from exslaves and their children, from former owners and their children and grandchildren, and from the general population—give a retrospective picture of what slavery was like for many of these people from the last decades of the nineteenth century to the middle decades of the twentieth. This information contradicts many of the common assumptions about the character of slavery as an institution that have been derived from the experiences of other societies, even of those elsewhere on the Kenya coast.⁴ But similarities also exist between memories of former slaves in the U.S. South and the recollections of exslaves on Lamu, which I recorded during my field work. In both cases, for example, many blacks regretted the passing of slavery, since they often believed that they were better off under their owners than in freedom under a wage-labor economy. Interviews with former slaves in the American South, carried out in the 1930s by the Federal Writers Project, reveal that house slaves, who had lived and worked in their masters' homes, had generally been less dissatisfied than field slaves, who had lived in huts apart and labored on the plantations.⁵ This same distinction appeared in the recollections of my Lamu informants.

Such a comparison may be skewed because of the differing age cohorts of the slaves interviewed. All of those to whom I spoke were younger than any who were interviewed by the Federal Writers Project; two-thirds of the former Southern slaves were over eighty and had been freed when the Civil War ended while they were still in their teens,⁶ but the average age of my exlave informants was less than sixty-five. In Lamu, as in the U.S. South, the passage of time may have dulled the sting of hardship. Yet their memories are a valuable historical source for our understanding of slavery and particularly for the material life of its victims.

A further point is semantic. Should people who have never legally been (in the secular sense) enslaved be referred to as "exslaves"? I have let the people under study answer that question. All of my Lamu informants defined themselves as

term "Afro-Arab" only for recent arrivals from Oman in the nineteenth century. Population figures fluctuated according to the numbers of people who were identified, or identified themselves, as Arab, Swahili, or Bajun. See Neville H. Chittick and Robert I. Rotberg, eds., *East Africa and the Orient* (New York, 1973), 43. M. Lofchie made a similar point for Zanzibar, while A. H. J. Prins has shown that the term "Swahili" can sometimes mean persons of slave descent; see Lofchie, *Zanzibar: Background to Revolution* (Princeton, 1975); and Prins, *Swahili-Speaking Peoples of Zanzibar and the East African Coast: Arabs, Shirazi, and Swahili* (London, 1960). And, although Randall L. Pouwels has suggested a new analysis of coastal history, he agreed that the term "Afro-Arab," as used in this essay, is valid, despite its association with Orientalists whose work he is revising; Pouwels, "The Medieval Foundations of East African Islam," *International Journal of African Historical Studies*, 11 (1978): 201–26.

³ Captain P. D. Henderson, "Precis of Correspondence relating to Zanzibar Affairs from 1856–1872," India Office Library, London, India Office Records 10R L/P, S/18B150a.

⁴ See, for example, Frederick Cooper, "The Problem of Slavery in African Studies," *Journal of African History*, 20 (1979): 102–26. Also see Cooper, *Plantation Slavery on the East Coast of Africa*. It would be important to have a comparative picture of slavery in Oman or Yemen in Arabia, or even in Saudi Arabia, where slavery was not officially abolished until 1962.

⁵ Norman R. Yetman, ed., *Life under the "Peculiar Institution"* (New York, 1970); B. A. Botkin, ed., *Lay My Burden Down* (Chicago, 1945); and John F. Bayliss, ed., *Black Slave Narratives* (New York, 1970).

⁶ Yetman, Introduction to *Life under the "Peculiar Institution"*, 1–2.

“free-born” or as “slaves.” Some, indeed, referred to themselves in interviews as slaves in the present tense. The terminology—with its underlying self-perception—shows that, at least where slavery is concerned, Islamic law triumphed over civil law in Lamu until the very recent past.

ABOUT 1800, FOUR TOWNS NEAR WHAT IS NOW THE KENYA COAST were nearly equal in size and importance—Zanzibar, Mombasa, Pate, and Lamu. Zanzibar in the nineteenth century became an independent sultanate and the center of a trade network stretching far into the East African interior. Mombasa became the main port town, with rail links serving both Uganda and Kenya. Pate fell into decline after joining Mombasa in an unsuccessful war against the combined forces of Lamu and Oman early in the nineteenth century. But Lamu remained much the same as before and is, perhaps as a result, the most conservative of those East African, “Arab” trading cities that survived into the second half of the twentieth century.⁷

No one knows the date of the earliest settlement of Lamu, although Arab settlements on the nearby island of Pate were among the earliest on the East African coast. Over the centuries, the settlers of Lamu included Shirazi, Portuguese, Turks, Indians (mainly the Bohras before the nineteenth century, Hindus and other Muslims later), and Arabs (among whom was the oldest acknowledged family, which came from Yemen). Later settlers included the Omani (who became much of the ruling class) and the Hadrimis.⁸ Only a handful of British nationals settled in Lamu, most of them in this century, and Britain maintained very few officials there. Given the preponderance of Afro-Arabs and their slaves in the population, the persistence of slavery was officially ignored by the resident British civil servants, who coexisted with the sultan’s appointed officials from 1907 on. “Afro-Arab,” as used here, is the Swahili *Arabu*, encompassing the descendants of Arab trader-settlers and the African women with whom they established sexual liaisons.

Lamu’s population from 1800 onward seems to have remained fairly stable at five to six thousand inhabitants, although estimates vary.⁹ Its ethnic composition in the late nineteenth century—Afro-Arab, Hadrami, Indian, African—also remained largely unchanged. The town was divided in several *mitaa* (“wards”), often settled by

⁷ Prins, *Swahili-Speaking Peoples of Zanzibar*, 110; and J. Spencer Trimingham, *Islam in East Africa* (Oxford, 1964), 88.

⁸ The Bohras were a Muslim spin-off sect who originated in northern India as Hindu traders. They kept to themselves and established separate mosques because some Hindu practices survived their Islamic conversion. The Bohras intermarried with African and, later, Afro-Arab families in their early years in East Africa. See Hakim Amiji, “The Bohras of East Africa,” *Journal of Religion in Africa*, 7 (1976): 27–40. For early coastal contacts with Arabia, see B. G. Martin, “Arab Migrations to East Africa in Medieval Times,” *International Journal of African Historical Studies*, 7 (1974): 367–90; and Pouwels, “The Medieval Foundations of East African Islam,” 201–26. Thomas H. Wilson, former coast archaeologist, has discovered that Pate interacted with various Indian Ocean cultures as early as the ninth century; Wilson to author, December 14, 1981; also see *The Standard* (Kenya), February 1, 1982.

⁹ Annual Report of the District Commissioners, Lamu, 1873–74, Kenya National Archives, Nairobi [hereafter, KNA], 2915, Box 105 ICS. J. de V. Allen claimed that Lamu’s population reached eighteen thousand at its peak, but his estimate is not supported by other scholars; Allen, “Swahili Culture Reconsidered,” *Azania*, 9 (1974): 105–38.

different ethnic groups. The northern, and older, half of the town, Mkomoni, was the home of most of the old Afro-Arab families. Settled by slaves, exslaves, and newcomers, the southern section, today's Langoni, grew in size during the nineteenth and twentieth centuries. An Arab fort built in the early nineteenth century lay near the division between the two halves.¹⁰ The present Friday mosque is at the extreme north end of town, a location that suggests it was once at the center of the old town.¹¹

In recent centuries, the peak of the social hierarchy has been occupied by old Afro-Arab families, who controlled agricultural land not only on the island but also, more extensively, on the mainland just a few miles away. By the late nineteenth century, some of the Indian newcomers began to rival the old Afro-Arab families in wealth, although the Indians made no effort to gain political power and kept within their separate and distinct communities.¹² During the nineteenth and early twentieth centuries, the old families continued to dominate the material life of Lamu, despite their waning financial position. Indian moneylenders became increasingly important as the fortunes of the old families declined. The Indians were the financial backbone of the nineteenth-century slave trade in Lamu, although not to the extent that Indian moneylenders were in Zanzibar and on the Swahili coast.¹³ Somewhat further down the social scale were newly arrived Arabs, mainly from the Hadramaut, who settled in significant numbers until the 1920s. They came to Lamu as poor men and, through hard work and thrift, eventually became traders, setting up small shops wherever space was available. The Hadrami lived in the southern end of town, which grew as their numbers increased. Hadrami-Arabs built their own houses and kept to themselves, for they were regarded as "strangers" by the old Afro-Arab families.¹⁴

¹⁰ Sheikh Ahmed Jahadhmy, personal interviews, Lamu, January–May 1980, February 1982; and Sharif Abdalla Salim el Hussein, personal interviews, Mombasa, May–June 1980, January and February 1981. Interviews with Jahadhmy occurred almost daily in 1980, and Jahadhmy read and offered several factual corrections to an earlier version of this essay.

¹¹ J. de V. Allen, *Lamu Town* (privately published; n.p., n.d.).

¹² Nizar A. Montani, "Uganda's Asian Refugees: Their Historical Background and Resettlement in Canada and the U.S.A.," *Kenya Historical Review*, 3 (1975): 27–46. The sultan of Oman (later Zanzibar) brought Hindus into Oman and then on to Zanzibar, where he employed them in his government, especially as customs officials. Indeed, Hindus came to Lamu through the sultan's patronage and introduction to the area. In the Lamu census of 1874, fourteen Bohra traders, one Khoja, and thirty-six Hindus appear; Annual Report of the District Commissioners, Lamu, 1873–74, KNA.

¹³ J. B. Kelly, *Britain and the Persian Gulf, 1795–1880* (Oxford, 1968), 411–51; and A. A. Skanda, personal interviews with Sheikh Ahmed Jahadhmy, Lamu, January–May 1980, January–February 1981. During both times, interviews with Skanda occurred almost daily.

¹⁴ Roger Frederic Morton, "Slaves, Fugitives, and Freedmen on the Kenya Coast, 1873–1907" (Ph.D. dissertation, Syracuse University, 1976), chaps. 3–4. Morton believed that social stratification was based solely on slave status and that Hadrami, newly arrived on the Kenya coast, "were considered free-born persons." Given "their Arab clan connections," Morton claimed, they "would have been able to attach themselves to coastal Arab families." *Ibid.*, 103. This was not, however, the case in Lamu. Even today, the Hadramis of Lamu are considered "outsiders" by the people who constitute the upper class within the town; Mohammed Ahmed, personal interviews, Lamu, February 1981; and various women informants, personal interviews, Lamu, February–May 1980, January–February 1981. (Because of rigid purdah still practiced among the women of Lamu's upper classes, I am not at liberty to publish the names of my female informants. If any scholar wishes to check my data, I shall be glad to provide the information in private communication, and, if any woman is interested in undertaking research among the women of Lamu, I shall be happy to provide assistance.) For a penetrating study of strangers in societies, see Frederic Barth, *Ethnic Groups and Boundaries* (Boston, 1969), esp. 11–15.

All three of Lamu's wealthier social groupings—Afro-Arabs, Indians, and Hadrimis—had slaveholding in common. It is, however, impossible to know how many slaves each group owned, for the number of slaves on Lamu at any time is unknown. Slaves did constitute at least half of the total population at emancipation. My exslave informants remembered either that they were born on Lamu or that they crossed over from mainland plantations. One exslave, who was sixty-seven in 1982, could recall (from oral tradition) one detail about the slave trade at the turn of the century—that slaves were imported from further down the coast or from the Kenya mainland to Lamu island. (She also remembered hearing that "Africans who were roaming around were captured.") Late in the nineteenth century, if not before, slaves were first brought to a trader's home, and the trader then invited his friends in to examine them. The best were kept for the old Afro-Arab families of Lamu town and island. Those who were rejected in this screening were removed to the slave market, where they were "sold like chickens"; some went to mainland plantations, others to Hadrimis. In some cases, families arrived together, and their members kept in contact with each other over decades on Lamu or, through good fortune, belonged to the same owner.¹⁵

The Hadrimis generally acquired slaves as soon as they were financially able. Their slaves were hired out to work as dock laborers, seamen on ocean-going dhows, fishermen, and porters in the town. (The only way to transport goods on Lamu was by donkey- or man-drawn carts, and some Hadrimis owned both donkeys and slaves.) Hadrimis commonly rented their slaves for profit rather than employ them in domestic service. Although the Hadrimis prospered from the returns on their slaves, their style of living remained basically unchanged. They dressed in simple clothing and lived in small houses, usually built of mud and wattle, near their slaves. The slaves lived in the same type of dwellings, although theirs were even smaller and less well furnished. As his family size increased, a Hadrami might employ his own slaves in building a larger house in their "spare" time. The Hadrami-owned slaves did receive some earnings, from which they paid their owners for their upkeep. Some informants said they received half of their pay plus food when working. Others remembered that slaves who were hired out to dhow owners received one-quarter of the profits, which were distributed roughly as one-half to the owner of the dhow, one-quarter to the captain, and one-quarter split among the seven to ten crew members.¹⁶ If the crew members were slaves, then their owners took one-half of the share they received. The balance had to feed and clothe the slave and his family. When profits were high, so was the take-home pay

¹⁵ Various women informants, including exslaves, personal interviews, Lamu, February–May 1980, January–February 1981; Ali el Maawiy, personal interviews, Lamu, February 1982; and Salim Kheri, personal interviews, Lamu, January–February 1981. Ali el Maawiy spoke for a very old woman he knew, who walked most of the way from the Tana River to Lamu as part of a small group of slaves; she later married into one of the oldest, most respected Lamu families. I conducted two extensive taped interviews with Salim Kheri, a very old exslave; he is, however, the "professional slave" in Lamu and has been interviewed by nearly everyone doing research on the town or mainland. Because he is quite poor, he is usually paid a small fee for giving an interview. I therefore checked every piece of evidence he provided against information from at least one other source. When his evidence conflicted with that available elsewhere, I tended to discount his. Other Lamu informants later confirmed that he may not always have been accurate; Ali Abdalla El-Maawiy and Ahmed Jahadhmy to author, September 5, 1981.

¹⁶ A. H. J. Prins's research confirms this division of income; *Swahili-Speaking Peoples of Zanzibar*, 69.

of the slave; when they were not, the slave received little except the displeasure of his owner.¹⁷

Hadrimis rented out slaves for other occupations, including fishing, which entailed the same division of proceeds. Other slaves worked as artisans, such as woodcarvers and assistants to gold- and silversmiths; woodcarving was a craft for which Lamu was noted, and fine metalwork was a substantial enterprise on Lamu before slavery was abolished. Some slaves were blacksmiths, and others made rough furniture for the poor or fine, well-crafted furniture for the rich. Slaves were also employed in constructing houses as well as dhows and smaller boats, which was a significant business on Lamu in the nineteenth century. Housing construction included digging sand from nearby beaches and bringing it to town by boat and to the construction site by donkey. Slaves also gathered stone and coral rag and made lime by burning coal.¹⁸

Indian moneylenders on Lamu financed the slave trade by importing slaves to Lamu and making loans to Afro-Arabs for slave purchases. They also owned some of the dhows used to ship slaves—illegally after 1873—to ports in India, in Arabia, and up the Somali coast of East Africa.¹⁹ Prosperous Indians bought slaves for use in their own households. Like the Hadrimis, Indians probably rented out some slaves for menial tasks, and Indian craftsmen employed slaves in their own shops.²⁰ But the Indians were apparently reluctant to use them as agents or salesmen.

Smaller numbers of other groups settled, either permanently or temporarily, on Lamu. Some free Africans—including the ethnic groups Pokomo, Oromo, and, at rare intervals, Somali—came to Lamu from the mainland. A few Bajuni exiles from neighboring islands lived on Lamu over several generations, but they were always identified by their place of origin and were considered strangers. Throughout the nineteenth century, some members of old Lamu families fell on hard times and hovered on the brink of poverty; even in these straits, they rarely entered the work force, preferring to live mostly on the generosity of their more prosperous kin.²¹ A few religious men, *sharifi* (“descendants of the Prophet”), came to Lamu more

¹⁷ Abdalla Salim el Hussein, personal interviews, Mombasa; Ahmed Jahadhmy, personal interviews, Lamu; Sharif Saleh Hassan Badawy, personal interviews, Lamu, February–June 1980, January–February 1981; A. A. Skanda, personal interviews with Jahadhmy, Lamu; and Athman Bakari, personal interviews, Lamu, January–February 1981.

¹⁸ James S. Kirkman, ed., “The Zanzibar Diary of John Studdy Leigh, Part I,” *International Journal of African Historical Studies*, 13 (1980): 297. Leigh observed female slaves carrying mortar for housing construction in Zanzibar, but my Lamu informants have no recollection of women performing such heavy labor in their town. Margaret Strobel found more female slaves than male slaves in Mombasa town, but the males do not seem to have been engaged in male trades, such as construction; see Strobel, “Slave and Free in Mombasa,” *Kenya Historical Review*, 6 (1978): 55.

¹⁹ Public Record Office, London, Foreign Office Records 84/2095, 84/1724–25.

²⁰ Accounts vary. The Bohra community, which is the only group of Indians of any size—about fifty to seventy-five in number—still on Lamu, was extremely reluctant to discuss its income and business activity, even historically. The Hindus are gone. One of the two remaining Parsi, Kiki Dastoor, provided most of the information included here, but he is a questionable source because of his age and declining health; Dastoor, personal interview, Lamu, June 4, 1980. His observations were, however, corroborated by local Afro-Arab elders. See Richard T. McCormick, *Asians in Kenya: Conflict and Politics* (Brooklyn, 1971), 14–15; Amiji, “The Bohras of East Africa,” 34; and Robert Gregory, *Indians in East Africa* (Oxford, 1971), 17–50. It was still possible, in 1980 and 1981, to observe some few old retainers serving as domestic slaves to the Indian traders. They worked in shops or, if they were too feeble, took care of the small Bohra children.

²¹ Lamu Political Record Book, Annual Report of the District Commissioners, 1919, Lamu, KNA; and A. A. Skanda, personal interviews with Jahadhmy, Lamu.

indirectly, from Arabia in the second half of the nineteenth century. Other settlers drifted in from Merka and Brava on the Somali coast, stayed a few years engaging in minor trade, and moved on. Still others came from the Comoro Islands further south in the late 1880s and 1890s and practiced medicine as well as offered religious instruction. Habib Saleh, one such immigrant from the Comoro, stayed; he opened a mosque school, later built a mosque with the help of slaves, and became a respected member of the community with a following mainly from the Hadrami faction of the town.²²

By 1840, when the Busaid dynasty under Sultan Seyyid Said transferred its government from Oman to Zanzibar, Lamu had already voluntarily abandoned its republican form of government and had become a protectorate of the sultan. Internally, the old families continued to dominate the town, although officials from Zanzibar—customs officials, who were usually Hindus, and a *liwali* (“governor”), who was usually a member of the sultan’s own family—began to play a role in governance.²³ Under Muslim law, a *kadi* (“religious judge”) ruled on legal questions with religious overtones, such as marriage and divorce. When the sultan introduced direct rule for Lamu, he also appointed the local kadis.

Lamu was known in the late nineteenth century, as today, as a religious center, and the sultan not infrequently appointed Lamu men as kadis elsewhere in his realm, including Zanzibar. Because the Omani had originally come to Lamu by invitation of the upper class, the Busaidi appointees were immediately included in the upper stratum of Lamu society, which was otherwise closed to outsiders.²⁴ Lamu women of the upper class were allowed to marry Busaidi and other Omani Arabs, whereas in the past they had only married within their own group, often within their family, to keep money and land in the same few hands. Intermarriage with the Omani Arabs meant that the Busaidi became “insiders”—were not regarded as strangers—in exchange for benefits to old Lamu families in Zanzibar.

Since Islam allows a man four wives if he can support them equally, the Omani

²² Some members of the old families, especially of families not of the upper class and, hence, religiously illiterate, attached importance to the sharifs. The sharifs were perceived as “holy” by the Hadrami and exslaves, and both groups brought their particular superstitions with them to the orthodox Sunni Islam on Lamu. Margaret Strobel has produced an excellent study of a similar phenomenon in Mombasa, but her work is somewhat deficient for Lamu because she leaned heavily on Abdul Hamid M. el Zein’s analysis; see Strobel, *Muslim Women in Mombasa, 1890–1975* (New Haven, 1979), 77–78 n. 88; and el Zein, *The Sacred Meadows: A Structural Analysis of Religious Symbolism in an East African Town* (Evanston, Ill., 1974). For my criticisms of el Zein’s work, see “*The Sacred Meadows: A Case Study of ‘Anthropologyland’ vs. ‘Historyland,’*” *Africa in History: A Journal of Method*, 9 (1982): 337–46.

²³ Clive, “A Short History of Lamu,” typescript, KNA (copy in the author’s possession), Appendix: list of liwalis; and Lamu Political Record Book, Annual Report of the District Commissioners, Lamu, 1919, KNA.

²⁴ Ylvisaker quite correctly pointed out that Lamu’s Afro-Arabs became concerned when the sultan abolished the slave trade, but, in quoting Frederic Holmwood, the resident commissioner in Zanzibar, on the murder of an Indian in Lamu, she left the impression that the majority of the residents were opposed to rule from Zanzibar; *Lamu in the Nineteenth Century*, 81, 96. Lamu informants noted that social, political, and, to some extent, economic relations were cordial and open-ended between Lamu and Zanzibar; only when the sultan became susceptible to British pressures first to end the trade and later to abolish slavery did ill will toward their ruler arise. In fact, some members of the sultan’s own family, resident in Lamu, realistically perceived that slavery and the trade were important to the town’s survival. But officials of both the sultan and Britain described Lamu’s Afro-Arabs as difficult to govern and given to intrigue. Sir G. H. Portal wrote, “The Arabs there have been naughty and want setting upon and I am to speak to them like a father—like an ill-tempered father,” and Clive referred to their “unsavory reputation” and “Oriental duplicity”; Portal to his wife, April 24, 1897, Lamu, Rhodes House Library, MSS AFR S105; and Clive, “A Short History of Lamu,” 12.

governor could keep a wife in Zanzibar and marry one or more women in Lamu while on assignment. Or, like Sultan Seyyid Said, he could have only concubines, whose children were regarded as legally equal to those born of a free wife. Descent among the Afro-Arabs was and is traced through the father's family; if the mother was free-born, her line was important, particularly if more prominent than her husband's, as in a marriage between a Lamu woman of the upper class and a Busaid newcomer. Slave lineage, by contrast, was generally traced through the mother's line, and her children belonged to her owner, not to her husband's master. In many instances, of course, husband and wife belonged to the same owner. As late as the 1950s, free men were still selling their domestic slave women to other free men; in such cases, children assumed the lineage of their free fathers.

From roughly 1850 to 1910, when Busaid influence was at its peak in Lamu, members of the upper class had substantial wealth from the income of their mainland plantations and, consequently, lived regally. Still, many lived beyond their means and incurred large debts with Indian moneylenders to maintain their opulent lifestyle. Among the families of the old upper class are the el-Maawiyi, the leading family of Lamu during the second half of the nineteenth century, the el-Bakari, a family that owned both plantations and dhows, the el-Husseini, a sharifian family of high status and little interest in religion, the el-Shatrys, a family originally from the Hadramaut but resident in Lamu long enough not to be confused with new arrivals, and the Jahadhmys, a family from Oman that settled in Lamu more than a century before the Omani came. Each of these families owned land on the Kenya mainland, where they kept a retinue of slaves. Contact between the slaves on mainland plantations and their owners on Lamu was minimal, since most slaves worked under the direct supervision of a *nohoa* ("overseer"), himself a trusted slave.²⁵ After emancipation, when revenue from the mainland plantations had ceased coming into Lamu, these upper-class families attempted to maintain their accustomed lifestyle and, as a result, plunged even further into debt. But, according to my informants, their principal income still came from the labor of their slaves, who turned over a percentage of their income to the heads of the households.

ONLY ON THE ISLAND ITSELF did interaction between Lamu's upper-class masters and slaves (or mistresses and slaves) take place—did the interrelationship of one group with the other affect the material life of both. On the small shambas behind the town, the slaves lived and worked in everyday contact with their masters. Weekend contact with their mistresses and the family children was frequent. The free wives and daughters of the old families were kept in total seclusion and rarely left their houses, except for occasional visits to female relatives or for religious observances in other homes. When their husbands or fathers took them on holiday to the shambas, however, women and children were allowed to walk around, to frolic, and to enjoy a measure of freedom from their town life in purdah.

²⁵ In a comparison of Zanzibar with Malindi, Cooper found Zanzibar's slaves working under the supervision of overseers on the large clove plantations, but Malindi masters sometimes supervised the work of their slaves directly; *Plantation Slavery on the East Coast of Africa*, 161, 173.

Mainland shambas were working plantations; grains and fruits produced by slave labor were sold for export. On the island, however, most food was produced only for local consumption—as it still is today. A single upper-class family typically owned two to three shambas, each with its own complement of slaves. The first shamba, called a *kitajuni*, was closest to town—often no more than a ten-minute walk from the town center—and was very small, sometimes as little as an acre or so. The second shamba, called a *makafuni*, was farther into the hinterland; it included some animals and produced beetle-nuts and some fruit and grain. The third shamba, called a *fuguni*, was farthest away and was also the largest; it had bigger herds and produced greater amounts of foodstuffs. A *kitajuni* was generally manned by one or two slaves and their families, a *makafuni* by four to twenty, and a *fuguni* by twenty to forty. On large shambas, slave families lived in houses at both ends of the plantation, and sometimes there was also a small cluster of houses in the middle; thus, the living arrangements on the shambas permitted a certain degree of privacy and provided space for separate garden plots for slave families. Shambas of five to six acres were worked by two to four slaves and their families, those as large as one hundred acres by about forty slaves. For the highly labor-intensive tasks of planting and harvesting, the labor of town slaves was used to augment that of their farming counterparts; otherwise, the shamba slaves and their families performed all of the farm labor.

All three types of shambas provided attractive environments as long as slaves worked at their upkeep. A lush tropical climate and sandy soil produced tall, stately coconut palms that, in turn, gave cover to a vast array of other fruit, nut, and grain crops. On smaller trees grew mangoes, which were a gourmet's delight—sweet, juicy, and succulent. Interspersed were papaya trees—tall and thin, with luscious fruit. Lime and orange trees provided citrus fruits. Trellises laden with vines produced grapes that had to be eaten as they ripened, because Islamic law prohibited making and consuming wine. Vegetables—peas, beans, sweet potatoes, carrots, hot peppers, onions, and even a short-grain rice (*kisuke*)—were grown on the island shambas.²⁶ *Kisuke* was used for rice cakes, a favorite at breakfast or supper. Most slaves, especially the newer arrivals, ate maize, also grown on the island shambas, or millet from the mainland. Wells, which the shamba slaves dug and maintained, supplied the water to irrigate the fruit and beetle-nut orchards and vegetable gardens.

Every large shamba had its share of poultry, goats, sheep, donkeys, and cows. Until the 1930s, some Arabian horses were bred, and camels provided power for grinding grain and extracting oil from coconuts. Donkeys were used for transporting heavy loads in town and carrying produce from the shambas to town. Children, even slave children, also rode the donkeys on special occasions.²⁷ Cows and goats were kept mainly for their milk. Calves were slaughtered according to Islamic law and custom and roasted over an open fire, especially for feast days, such as Idd al Fitr, which marked the end of the fasting month of Ramadan. (On Idd al Fitr,

²⁶ A. A. Skanda, personal interviews with Jahadhmy, Lamu; Mohammed Jahadhmy, personal interview, Lamu, May 1980; and exslave and Afro-Arab women, personal interviews, Lamu.

²⁷ A. A. Skanda, personal interviews with Jahadhmy, Lamu.

masters also gave their slaves presents of new clothing or money.) Chickens provided eggs, and hens were not slaughtered for the table until they had stopped laying. For elite families, the meat of goats, sheep, and sometimes calves complemented a diet otherwise marked by fish fresh from the sea—caught daily by slave labor and sold in the town market. Townspeople who had no shambas could purchase animals raised for profit on shambas. The upper class often donated animals to the mosques as a religious gift and supported the mosques financially as well, including providing the slave labor for construction and maintenance.

The coconut was a crucial plant to Lamu's economy. Every conceivable part of the coconut palm was used—from the frond, which was dried and matted for bags, rugs, and roofing, to the nut itself, which was used for cooking oil and finally for fuel.²⁸ Every slave family was dependent on the coconut for food, furniture, work, and fuel. Each shamba had separate plots, usually near the slave quarters, reserved solely for each slave family. Women almost always worked these plots while men grew the produce for the use of their masters and for sale in town.²⁹ Although the slaves enjoyed a rich, plentiful, and nourishing diet, they also worked very hard. Men, boys over ten, and sometimes wives, labored from sunup to sundown—climbing, bending, planting, picking, cultivating, and carrying water in vessels hung from poles over their shoulders. In coconut season, men did the hazardous work, climbing tall trees to cut down the fruit. They then gathered the coconuts and carried them to town, walking through heavy sand and, when no donkeys were available, bearing the produce on their backs or heads in loaded homemade mat bags. A trusted older slave served as an overseer on each large island shamba, as had been the case earlier on the mainland. His job entailed cajoling the work force into unceasing activity during harvest. Except during the heavy rains, which normally occur in May and June, either the master or one of his sons visited the shamba daily. These frequent contacts established a rapport on the island shambas that was not possible on the larger and more distant mainland plantations.³⁰

Male slaves worked six days a week.³¹ On Friday, men were free to attend

²⁸ *Ibid.*; Ahmed Jahadmy, personal interviews, Lamu; and free-born and exslave women, personal interviews, Lamu. Coconut was widely used for medicinal purposes, especially as a curative for bladder infections. Another by-product of the coconut was a fermented drink called *tembo*, which was made by tapping the coconut before the nut developed, draining the sap, and processing it. Old family informants denied that anyone made a fermented "liquor," which was illegal under Islamic law, but reports of the district commissioners suggest that the practice was common, especially after slavery was legally abolished and the fortunes of the upper class declined. See, for example, Annual Report of the District Commissioners, December 31, 1931, Lamu, KNA, BC/Coast I/1238.

²⁹ Much of the material in this section comes from extensive interviews with exslave women and the "professional" exslave Salim Kheri, conducted January–February 1981 and February 1982, Lamu.

³⁰ The district commissioner for Lamu reported in 1909 that the oldest son supervised the mainland plantations; KNA, LMU/11. Ylvisaker drew the same conclusion; *Lamu in the Nineteenth Century*, 82. Lamu informants said the master himself dealt with his overseer, but they suggested that on the island shambas the sons not only dealt with the slaves but sometimes even joined slaves in working during the peak of the harvest season.

³¹ Fred J. Berg found a slightly irregular pattern of slave workdays: some slaves worked six days a week, and some five; Berg, "Mombasa under the Busaidi Sultanate: The City and Its Hinterlands in the Nineteenth Century" (Ph.D. dissertation, University of Wisconsin, 1971), 175. Also see Ylvisaker, *Lamu in the Nineteenth Century*, 58; and Cooper, *Plantation Slavery on the East Coast of Africa*, 153–212. Cooper provided an excellent description of slaves at work on the coast farther south, and his informants differed among themselves as to the number of days off they had per week.

mosque. They were also free to carry out jobs for themselves and their families. Friday services at the mosques in town ran from 9:00 A.M. until 2:00 P.M.; there the men recited prayers and passages from the Koran. Probably most slaves had only a nominal acquaintance with the Koran, since mosque schools were few—two or three at most—and, because they were located in town, were inaccessible to shamba slaves except on their days off. Informants uniformly agreed that masters instructed shamba and town slaves in the basic tenets of the religion. During the month of Ramadan, slaves continued working but fasted from sunup to sundown. Masters also encouraged the construction of small mosques in the form of huts on the shambas, although only the men attended prayers there. In Lamu, as in Oman and Zanzibar, women were not allowed to pray in the mosques.

Lamu town had no army, although in the nineteenth century Baluchi soldiers representing the sultan were stationed at the fort there. But, as earlier under the republic, each free family had a slave army. Lamu men armed and drilled their slaves on the island shambas to create a militia for use in wartime. They prepared for war against the neighboring island of Pate or the Sultanate of Witu, both of which were regarded as enemies by slave and free men alike.³² At least once in the 1870s, however, Lamu men took their slave armies to the mainland to recapture a village of runaways. The runaways, joined by local allies, defeated the Lamu force with a “rain of arrows,” forcing it to beat a hasty retreat to Lamu. As Frederick Cooper found elsewhere on the Swahili coast, slaves were unreliable for recapturing other slaves.³³ Slaves and free men alike used bows and arrows, spears, and, when available, muzzle-loading muskets. Sometimes, however, the masters carried spears or guns while their slaves were only armed with bows and arrows. Both masters and slaves still drilled on the island shambas at least until the late 1930s, although the British presence made warfare between island communities no longer possible.

The burden of station fell more heavily on the female shamba slave than on her town counterpart. Informants disagreed about how marriages were contracted among Lamu’s shamba slaves. Some said marriages were approved by the master, as in the town, but were hazy about details. Two personal narratives illuminate the general statements. One woman remembered walking from a plantation near the Tana River to the coast, a distance of about twenty-five miles, and traveling to Lamu, where she was placed on a shamba belonging to a member of the el-Maawiyi family. On that shamba, she met and married her first husband, also a slave. Permission for this marriage was granted by the master. The second woman was born in a house in Lamu town and married her first husband, also a town slave (but one who worked on the farm when the demand for shamba labor was high), following an arrangement with the husband’s owner. She moved to the house of

³² Ahmed Jahadhmy, personal interviews, Lamu; Salim Kheri, personal interviews, Lamu; and A. A. Skanda, personal interviews with Jahadhmy, Lamu.

³³ For material on slaves as soldiers, see Ylvisaker, *Lamu in the Nineteenth Century*, 156; and Cooper, *Plantation Slavery on the East Coast of Africa*, 190–95. The practice of arming slaves in Lamu continued into the 1930s; A. A. Skanda, personal interviews with Jahadhmy, Lamu. Frederick Cooper suggested to me that the term “slave army” may be too strong, although he did find that “military service was part of the generalized expectations masters had of their dependents”; Cooper to author, November 1981.

her husband's master, dissociating herself permanently from her former owner. Her previous owner, a Busaidi related by marriage to her new owner, was compensated when a female slave owned by his relative married one of his male slaves. The exchange of persons was even, with no money passing between the two slaveowning families.³⁴

In his study of Mombasa Fred J. Berg found that masters sometimes bought wives for their slaves.³⁵ On Lamu, this rarely occurred. The slaves became part of the masters' extended families and tended to marry within their own "families"; sometimes slave marriages were arranged between owners of neighboring shambas, most of whom were themselves related by marriage if not by blood. *Washambala* ("new slaves") were considered outsiders, even if they came from the mainland plantations of Lamu's residents, and were not eligible to marry *wazalia* ("old slaves") resident either in town or on the island shambas. The *washambala* were first tested for work habits and loyalty; they also, of course, had to learn Swahili. Then they could gradually be incorporated into the slave society on the island.³⁶

Among the slave women's duties was herding animals on fenceless shambas. The women also helped their husbands gather the harvest, planted and picked the fruit and vegetables for their own families, and gathered firewood to prepare their meals. They ground rice, maize, or millet each morning and daily carried from the well to the house all water used by their families. Because slave families worked hard in intense heat, their clothes had to be washed often. That burden, too, fell to the women. They had sole care of their children, who, when old enough, helped out with family chores.

Because they had little contact with the world beyond the shamba, slave women there knew even less of Islam than their spouses. While their husbands enjoyed a Friday break from labor, the women's duties to the husbands, families, and households continued without letup. They dried palm fronds and made brooms, used to sweep the dirt floors of their small, mud-and-wattle houses. They dried bean and peas to tide their families over the months with little rain and less harvest. Women on the same shambas, or even women on neighboring shambas, sometimes helped one another—which not only lightened individual women's loads but also provided opportunities for visiting. According to one exslave, no matter the time of day, or the day of the week, it was "work, work, work."³⁷

The one avenue of escape and upward mobility for Lamu slave women was concubinage. The history of Lamu is full of stories of concubines who won the attention of their masters or masters' sons, gave birth to their children, and became

³⁴ As late as 1955, a daughter of this marriage was sold into concubinage to an important local sharif. The mother, one of my exslave informants, claimed the sharif and the girl's father arranged the marriage. Other informants, members of the original owning family, claimed that, instead, they made the arrangements, for which they received a token payment from the sharif. In general, the sharifs, especially some descendants of Habib Saleh, were influential in keeping slavery alive in Lamu. These sharifian descendants believed that only masters, never the civil authorities, could free their slaves, and they preached this strict constructionist interpretation of the Koran to the slaves who were among their followers.

³⁵ Berg, "Mombasa under the Busaidi Sultanate," 170.

³⁶ Exslave and Afro-Arab women, personal interviews, Lamu; and Ahmed Jahadhmy, personal interviews, Lamu.

³⁷ Exslave midwife and traditional healer, personal interviews, Lamu, February 1981, February 1982.

free. Indeed, there is no other easy explanation for the rise of the Afro-Arab class. Masters and their sons commonly caught sight of comely women on the shambas and enjoyed sexual relations. If the women became pregnant, the men were legally obliged to elevate them to free status. These former slave women essentially became cowives, and their children were considered equal to those born of the free, upper-class wives.³⁸ Those women who remained in bondage—that is, the majority—were able to participate in the cash economy through small cottage industries, which included weaving mat bags and making elaborate *kofia* (“woven hats”) for the men, who, according to Islamic custom, kept their heads covered at all times.

Fifty to sixty shamba owners had second homes on their plantations.³⁹ Some were two-story stone houses such as those found in town; others were more modest but, even so, contained two or three rooms in which the women—older, married women especially—could be secluded after they reached the shamba under the cover of a large, tentlike piece of cloth (*shiraa*) carried by their female slaves. Although seclusion was not as rigid on the shambas as it was in town, some privacy between the sexes was generally observed, and the women always needed a place of retreat when neighboring men visited their menfolk.

All of the slave quarters on Lamu island shambas have disappeared or fallen into such disrepair that direct evidence of their construction is not available. But indirect evidence can be found in the present homes of many Lamu exslaves, which, their occupants told me, are like those that existed on the shambas earlier in the century. These houses, some of which I visited, have mud-and-wattle walls with coconut frond roofs, and their floors, while spotless, are of mud or sand. In one such house, the front room contains a bed with benches on two sides of the room; behind is a curtained-off second room, a storage area; adjacent to that is a small kitchen, where, during slavery, the children slept. Another house—also owned by an exslave, who described it as much like the one she occupied on the shamba—has a small entry hall with benches and a small chair. Here they entertain visitors and eat their meals. To the left, behind the curtain (used instead of a door in all Lamu houses, whether for the rich or the poor) is a small room with a bed. All of the furniture is locally made (in the past, by slave labor) and quite severe. Seats and beds have wooden frames for tightly woven, coconut-frond caning, and the beds sometimes have coconut mats for mattresses. When other furniture is present, it consists of log benches. In the back of this house is a small kitchen with a few utensils, but the

³⁸ A. A. Skanda, personal interviews with Jahadhmy, Lamu; Sharif Abdalla Salim el Hussein, personal interview, Mombasa; Ahmed Jahadhmy, personal interviews, Lamu; and Afro-Arab women, personal interviews, Lamu. One of these informants was an exslave and former concubine, later *suria* (“freed cowife”), and married into a Busaid family; the personal interview with her took place in February 1982. El Zein incorrectly stated that concubines became something between slaves and free women and that, as a result, the children of such liaisons did not inherit equally; *The Sacred Meadows*, 31. Strobel repeated el Zein’s error, citing *The Sacred Meadows*, in her *Muslim Women in Mombasa*, 44 n. 2. Also see Richard F. Burton, *Zanzibar City, Island, and Coast*, 2 vols. (London, 1872), 1: 379. All of my Lamu informants, including the women who became *suria*, stressed the legal equality of a *suria* and a free-born wife as well as of a *suria*’s children and the children of a free-born wife. Since descent was and is traced only through the male line, the status of the woman a free man married mattered little, except insofar as wealth and possession did—or did not—stay in the family. Arranged first marriages tended to protect the transference of wealth; second and later marriages seem not to have involved the same degree of protection.

³⁹ Afro-Arab women, personal interviews, Lamu; Mohammed Jahadhmy, personal interviews, Lamu; and Ahmed Jahadhmy, personal interviews, Lamu.

actual cooking takes place outside over charcoal; earlier in the century the fuel was firewood. This house had three inhabitants from three generations—an old exslave man, his daughter, and her daughter.⁴⁰ Co-existing with the family are mosquitoes, fleas, and a thick cover of flies. On the shambas, some slave houses also had a small room with a latrine dug down into the sand. This room, if present, also contained water pots, which were used before the ritual of prayer. In other houses, the water pots were stored wherever the occupants slept, and, for a toilet, the slaves simply used some private place outdoors.

The master provided clothing for his slaves once each year. Female workday dress was the *kanga*, worn in sets of two. A *kanga* was a piece of cloth, large enough for a woman to wrap herself from the armpit to below the knees; a second piece was used to cover the head and upper body. (Women covered their heads and shoulders when they prayed.) Male slaves were issued two different items of clothing. For work, each male wore a piece of heavy material (*kikoi*) wrapped around the waist and, reaching below the knees, looped through the legs; the garment thus resembled a somewhat loose pair of shorts. For sleep, he wore a *leso*, made of softer material, more like that used in the woman's *kanga*. Male and female slaves were expected to obey the Muslim standard of modesty, keeping their bodies covered at all times including during sexual intercourse.

Female slaves who were enterprising enough to earn extra money sought to beautify themselves by buying small gold rings for their noses. Even the poorest aspired at least to a silver nose ring. Gold bangles were a must among upper-class women. Some slaves copied this fashion with silver bracelets—or even wooden copies—on their arms and legs. Jewelry, no matter how primitive, was as important to the slave woman as to her mistress, especially for a town slave, who had more leisure time and therefore more opportunity to enter a limited cash economy. Even slaves who wore no shoes coveted jewelry, a phenomenon still true among those who consider themselves exslaves.

At least until the 1940s, farm tools were few and primitive. A long stick with a small, crude metal blade was used for opening coconuts. Introduction of the *panga* ("machete" or "cutlass") after the Second World War provided a substitute for those few who could afford one. Another long stick, with a straight piece of metal tied to its end, served as a hoe. The slaves dug shamba wells with whatever implements they could fashion for the purpose.⁴¹

If the shamba had a *nohoa*, he supervised all slave activities and was responsible for discipline. According to both exslave and upper-class informants, punishment was limited to harsh commands. On those rare occasions when male slaves fought among themselves or were haughty to the *nohoa*, he whipped them with a stick. Females were not beaten by the overseer but frequently were by their husbands.⁴²

⁴⁰ Personal observations, February 1981 and February 1982, Lamu. For a theoretical analysis of the space and layout of homes and yards, see Sidney Mintz, *Caribbean Transformations* (Chicago, 1974), 225–50.

⁴¹ Exslave women, personal interviews, Lamu; and Salim Kheri, personal interviews, Lamu. Some of my informants demonstrated for me how these tools were made and used in February 1981. Also see Ylvisaker, *Lamu in the Nineteenth Century*, 47.

⁴² Exslave women, personal interviews, Lamu; and Salim Kheri, personal interviews, Lamu. Frederick Cooper has provided a detailed account of the treatment of slaves in Zanzibar and on the coast south of Lamu; *Plantation Slavery on the East Coast of Africa*, 164–67. Lamu informants, exslave and free, all stated that no stocks

The distance between masters and slaves on the mainland plantations seems to have made for harsher treatment, greater discontent, and, hence, more runaways than was the case on the island shambas. Arabs were generally regarded as kind to household slaves and paternalistic toward shamba slaves, with whom they enjoyed a close relationship. Bishop Steere observed of the Arabs on Zanzibar in the 1860s, "We have not now to deal with anything at all which resembles the Old West Indian, or the most recent United States slavery. The English were infinitely worse masters than the Arabs are, and even now Englishmen generally have a very much less kindly feeling towards a free Negro than the Arabs have towards their slaves."⁴³ Yet Lamu informants who had traveled to or lived in Zanzibar claimed that slaves were better treated in Lamu than in Zanzibar.

The shamba and town female slaves had a low fertility rate from the late nineteenth to the mid-twentieth century.⁴⁴ An old exslave attributed this phenomenon to the suckling of babies, which inhibited conception, and to abortion, which was widely induced by eating certain local herbs. She stated emphatically that no artificial, mechanical devices were used in aborting a fetus.⁴⁵ In addition to abortion and the suckling of infants, the change in disease environment for new slaves may also have affected their reproduction rate, but it does not seem to have increased the death rate on the island. Three complaints were common among slaves and free men in the twentieth century, when documentation is available. One was respiratory problems at the change of the monsoon, diagnosed variously as colds, bronchitis, tuberculosis, or pneumonia. (In some cases all of these diagnoses may have been correct.) The second was the continuous variety of stomach ailments, no doubt caused by drinking contaminated cistern water. The third was infectious diseases, which were treated by a variety of local herbs. Malaria, of course, topped

were used for imprisonment in the town but that slaves were sometimes placed in the fort. Punishment, they agreed, was not harsh; it usually consisted of a few lashes. Frederick Cooper found that the treatment of slaves was harsher in Malindi; see Cooper, "The Treatment of Slaves on the Kenya Coast in the Nineteenth Century," *Kenya Historical Review*, 2 (1973): 95.

⁴³ Edward Steere, Preface to H. A. Frazer *et al.*, *The East African Slave Trade and the Measures Proposed for Its Extinction* (London, 1871). Leigh found that in the Zanzibar slave market, however, Arabs exhibited insensitivity toward slaves, with whom the purchasers had formed no attachment: "The Arabs went round examining them as they would cattle"; Kirkman, "The Zanzibar Diary of John Studdy Leigh, Part I," 286. Morton believed that blacks (Africans) were considered inferior because of their skin color; "Slaves, Fugitives, and Freedmen on the Kenya Coast," 107. Frederick Cooper disagreed: "Negroid features are not the clear and highly visible indication of inferiority they are in the Americas"; "Treatment of Slaves," 97. Lamu informants, including exslaves, regarded the treatment of slaves on Lamu island as more humane than in Zanzibar or on the mainland; they rejected the notion of harsh treatment on the basis of color. For Arab treatment of slaves in Arabia, see Kelly, *Britain and the Persian Gulf, 1795-1880*, 411-12. Also see Burton, *Zanzibar City, Island, and Coast*, 1: 368-460 and 2: 491-92.

⁴⁴ Burton, however, claimed that the birth rate was high, but he gave no figures; *Zanzibar City, Island, and Coast*, 2: 495.

⁴⁵ Exslave midwife, personal interviews, Lamu. I was given a sample of one herb used for aborting a fetus; the herb tasted like anise and was used in smaller amounts by Lamu Indians as a condiment. The old midwife claimed that, if this herb failed, other herbs, which were also consumed, were tried. The herb I tasted was also used to cure stomach aches and other problems related to the digestive system. Many Cuban women apparently practiced abortion by drinking a brew concocted from wild herbs; Franklin Knight, *Slave Society in Cuba* (Madison, Wisc., 1970), 78. A. J. H. Prins also wrote of the "low reproduction rate" among slave peoples; *Swahili-Speaking Peoples of Zanzibar*, 24. E. H. Richardson, M.D., said that, although he was skeptical about the efficacy of eating certain herbs for abortion, such a result was possible. Richardson knew that in the nineteenth century some American women believed that drinking the old Lydia Pinkham Compound brought about abortion. Richardson, personal interview, October 12, 1981, Baltimore, Md.

the list of illnesses that afflicted all residents on the island. The world-wide flu epidemic in 1918 (*malangi langi*) struck free men and slaves with equal severity, killing off proportionally more females than males, perhaps because women were not allowed access to even the primitive government medical services then available.

Upper-class Afro-Arab informants claimed that prostitution did not exist among Lamu women. But later medical records reveal that men who visited the local clinic had a high incidence of venereal disease.⁴⁶ According to exslave informants, some town slave women were prostitutes, whose masters rented them out to visiting seamen and kept a share of the profits. Homosexuality among young, unmarried men was common, which also contributed to the high incidence of venereal disease, and it is not unlikely that young slave boys engaged in homosexuality with their young masters as well as with visiting seamen for a price.⁴⁷

Most Lamu two-story stone houses, built by slaves for their masters in the nineteenth century, were large and grand in style and could accommodate a host of people.⁴⁸ The ground floor was used by men for meetings or for housing visiting male guests. Upstairs, off the open courtyard, were a series of galleries, one leading back from another, and growing darker from one room to the next. The harem, or rooms for the women, were in the back galleries. In some houses, the master's bedroom and its bath were at the extreme back of the house; in others, all rooms opened off to one side of a long courtyard. The rooms at the far end were reserved for the women of the house and their female visitors. Children, separated by sex, slept in beds along the walls in the galleries or in rooms especially designated for them. If a man kept more than one wife in his house, he was supposed to shift from bedroom to bedroom on a nightly basis. If each of his three or four wives had her own home, he was obligated, according to Islamic law, to shift from house to house and sleep with each wife an equal number of nights. Slaves slept on floor mats downstairs, sometimes in the kitchen area, or in the open courtyard, except during rainy season, when they were forced to seek shelter inside the house.

Town slaves performed all household tasks except the cooking, which was usually done by the wives. (An informant who was an exslave until elevation to *swia* ["the

⁴⁶ The reports of the various British district commissioners contain records compiled annually of all instances in which the medical service treated males for venereal disease, and the number of cases was always high; see Annual Reports of the District Commissioners, Lamu, 1909–39, KNA. Burton reported a high incidence of venereal disease among slaves, which he attributed to "promiscuous intercourse"; *Zanzibar City, Island, and Coast*, 2: 183.

⁴⁷ Exslave women, personal interviews, Lamu; Ali A. Jahadhmy, personal interview, Mombasa, June 1980. Mohammed Busaid remembered that, years ago, Arab dhows regularly called in Lamu; Busaid, personal interview, Lamu, February 1982. Young boys of the upper class were not allowed to go to the waterfront when the dhows were in port because their parents feared the adolescents would be seduced by sailors who were known to give money or presents to young boys in exchange for sexual favors. For additional information on prostitution in the Lamu area, see Janet M. Bujra, "Production, Property, Prostitution: 'Sexual Politics' in Atu," *Cahiers d'études africaines*, 65 (1973): 13–39.

⁴⁸ The stone house is an exceptional phenomenon in East Africa and has been studied in depth. For brief but interesting descriptions of Lamu's stone houses, see Usam Ghaidan, *Lamu: A Study in Conservation* (Nairobi, 1976), and *Lamu: A Study of the Swahili Town* (Nairobi, 1965), chaps. 3, 4. Chapter 3 of Ghaidan's earlier work deals especially with the spatial organization of Lamu's stone houses. Also see J. de V. Allen and Thomas M. Wilson, *Swahili Houses and Tombs of the Coast of Kenya* (London, 1979). Although the structural descriptions of the houses are accurate in this pamphlet, Allen's discussion of the social functions of the houses is almost entirely incorrect. (Allen wrote the section on houses, Wilson that on the tombs.)

status of free wife"] in the 1930s emphatically stated that she did no further cooking for her then-husband.) The male slaves carried water from nearby wells to the houses, unless the houses contained cisterns, and periodically carried the ornate furniture—chairs, beds, benches—from the house to the seafront to be soaked in the seawater to rid it of insects (the woven matting was a breeding ground for fleas and bedbugs). Male slaves also carried firewood, fruit, and produce from the shambas to the townhouses.⁴⁹

Mosques were the public meeting places for the free men of Lamu. After prayers, especially in the early evening, they remained there to discuss events of the day, politics, or any other topic except their women, whom they never mentioned in public. Because the mosques were, and are, important to Lamu male society, their cleanliness and upkeep was important. Male household slaves were loaned to clean, paint, and generally maintain the mosques. Slaves were expected to work daily in family-owned mosques, seeing, for instance, that water jars were full for the ritual washing that took place five times a day.⁵⁰ Various rules applied to the slaves' worship. In some family-owned mosques, slaves were unrestricted, could worship freely at all times; in other mosques, slaves were allowed to worship at specific times, generally when upper-class men were not at prayer. In one mosque, Pwanee, the oldest in Lamu town, slaves were denied the right to pray at all times.⁵¹

After their daily chores were finished, male town slaves were free to hire themselves out and keep all of their earnings. A slave could save enough to buy his freedom and that of his wife and children. Or he could gradually use his earnings to build a home of his own in the southern end of town. Land and property rights were complicated, but in general a squatter was declared the owner of the tiny parcel of land on which he built his house. Sometimes a master donated a bit of land to a slave; or, if the land the slave built on was considered part of the property of a particular mosque, he was charged a very small rental fee, which was used to help defray the expense of maintaining the mosque.⁵²

Town slaves of the Afro-Arabs enjoyed a range of jobs and benefits not available to those on the shambas. Some began their service in business or in shipping and rose to positions of importance themselves. Until legal emancipation, and perhaps as late as 1910, some Lamu slaves also became slaveowners. A trusted slave who started as a seaman on his master's dhow on occasion became its captain, sailing with his crew of other slaves to ports as far away as Arabia, India, Zanzibar, and Pemba or up and down the East African coast to any of the several ports where the master had established trade relations. These slave captains enjoyed the authority to conduct all negotiations in their masters' interest and were well rewarded when

⁴⁹ Women informants, personal interviews, Lamu; and Ahmed Jahadhmy, personal interviews, Lamu.

⁵⁰ Mzee A. A. Skanda, father of A. A. Skanda, was reputed, at age ninety-five, to be the second oldest Afro-Arab in Lamu town in 1982. A very religious man, he refused a personal interview with me. He did, however, consent to a taped interview with Saleh Hassan Badawy, who turned the tape over to me. I posed all questions, and Mzee Skanda responded to each of them in turn. The tape is valuable for what it contains, but what is lost is the benefit of follow-up questions. Mzee Skanda, taped interview with Badawy, April 1982, Lamu; and Ahmed Jahadhmy, personal interviews, Lamu.

⁵¹ A. A. Skanda, personal interviews with Jahadhmy, Lamu; and Ahmed Jahadhmy, personal interviews, Lamu.

⁵² Mzee Skanda, taped interview with Badawy, Lamu; Ahmed Jahadhmy, personal interviews, Lamu; and exslave women, personal interviews, Lamu.

they returned with ample profits.⁵³ Presumably, they were punished or removed from authority when they returned empty-handed.

Lamu town slaves were more likely to have been better acquainted with the tenets of Islam than were their shamba counterparts. Most slaves who came to Lamu town became Muslim, but, as on the shamba, they also retained many of their African religious beliefs, rituals, and medical practices.⁵⁴ Their children were brought up under the supervision of the master and mistress, who were likely to have been devout themselves and who taught religion to the slaves, so that the second and following generations became more orthodox, though still theologically uninformed. The syncretism in religious beliefs and cultures overlain with Sunni Islam has persisted in Lamu until the present time; in some cases, the old Afro-Arab families adopted the African beliefs and customs in addition to their own Islamic practices. This combination of religious traditions was especially prevalent among children born to fathers from the old families and their concubine wives—in other words, among the majority of the Afro-Arab population in today's Lamu. Indian practices, too, such as putting kohl on and around the eyes of babies to protect them from blindness or eye disease, worked its way into the culture of Lamu's upper class.⁵⁵

Masters' perceptions of their slaves ranged all the way from invisibility—or "nothingness," as one informant put it⁵⁶—to something like extended kinship of a lower order. The Koran admonished slave owners to be paternal toward their slaves, and in some families this policy was practiced within the limits of normal relations between client and patron. Emotional ties between masters and slaves were contained within clearly defined modes of behavior. On feast days, slaves were given presents, alms, and special privileges. But slaves were expected to be absolutely submissive to their masters at all times, although Lamu's slaves, unlike those in Zanzibar, were not compelled to crawl on their hands and knees to and from their masters' presence.⁵⁷

No racism, as we know it, existed in Lamu. Centuries of intermixture between Africans and others had eliminated any sense of color class.⁵⁸ There was, however, a rigid consciousness of social class that gave to slaves the lowest status, or "nothingness," as social beings. Town slaves were, on the whole, better treated than

⁵³ A. A. Skanda, personal interview with Jahadhmy, Lamu; Ahmed Jahadhmy, personal interviews, Lamu; and Afro-Arab women, personal interviews, Lamu. Burton reported that slave captains enjoyed wide responsibilities in Zanzibar; *Zanzibar City, Island, and Coast*, 1: 466. Also see Cooper, *Plantation Slavery on the East Coast of Africa*, 168–69. Cooper found that in the 1890s slaves farther down the coast also owned property, which on their death they had to pass on to their masters; "Treatment of Slaves," 94, 98. Salim Kheri as a slave worked on a ship with a slave *nuhoa*; personal interviews, Lamu.

⁵⁴ Mzee Skanda, taped interview with Badawy, Lamu.

⁵⁵ A. A. Skanda, personal interviews with Jahadhmy, Lamu; and Mohammed Fati (a local Bohra trader), personal interview, Lamu, June 1980.

⁵⁶ Mzee Skanda, taped interview with Badawy, Lamu.

⁵⁷ Ali Jahadhmy, personal interview, Mombasa, June 1980; and Sharif Abdalla Salim el Hussein, personal interview, Mombasa, February 1981. For a glimpse of the psychological relationship between master and slave in Lamu, see Paul G. Forand, "The Relation of the Slave and the Client to the Master or Patron in Medieval Islam," *Journal of Middle Eastern Studies*, 2 (1976): 59–66. For a comparison with slaves in the U.S. South, see Eugene Genovese, *Roll, Jordan, Roll: The World the Slaveholders Made* (New York, 1972), 327–65; "Life in the Big House."

⁵⁸ Cooper confirmed the lack of color bias for Mombasa and Malindi; *Plantation Slavery on the East Coast of Africa*, 266–69. Also see note 40, above. For other views on this same theme, see Patricia Romero, "Slave

their shamba counterparts, because their relationships to their masters were closer, but incidents of cruelty were not unknown even in town. At times, the slaves tried to escape from their masters' households. To escape from Lamu was not, however, easy, since slaves were readily identifiable. Some did manage successfully to stow away on dhows. Immediately following emancipation, two or three sometimes joined together, stole a small fishing boat in the harbor, and made for the mainland, where eventually they joined *watoro* ("runaway slave") villages. Some made their way to Witu on the mainland and found acceptance in the sultan's army.⁵⁹

Laws governing slave behavior on Lamu were not so harsh as stipulated in the Sharia. But, if a slave killed an Afro-Arab, he was condemned to death by torture. His master was then forced to pay a sum of money, in the amount determined by the kadi, to the family of the deceased. If a slave ran away and was recaptured, he was punishable by the loss of a limb, usually a hand; the same punishment could be meted out for stealing. Lamu informants, both exslave and free, said that these practices, though ordained by the *sharia*, were rarely invoked—at least in living memory.⁶⁰

Because of their status in the household, male slaves were able to move freely in and out of the harem, where the women were closely secluded from outside contact with males other than members of their families. Female slaves also circulated freely within the house as well as about town. Although some informants maintained that domestic slaves—both male and female—were never sold, others held that slave women were sold to upper-class men who had seen them in the marketplace and wanted them as concubines. Lamu men had a preference for Oromo women, who were sold in the market there for concubinage.⁶¹ Fathers often gave their young, prepubescent sons concubines for pleasures until they could arrange appropriate marriages. This practice prevailed among some old Afro-Arab families at least into the 1930s. It was common for a man, having married a cousin or other member of his family, to take a slave mistress, or several, from his parents' household and keep them as concubines. We do not yet know enough about the heart versus the pocketbook (or politics) in arranged marriages at other times or places to compare the success of such alliances on Lamu with that in other societies. My Lamu informants—men and women, free and exslave—all believed that the special concubine, not the wife, most often captured the heart of the master. The extremely high divorce rate from first, arranged marriages seems to bear out the assumption that such alliances were generally not happy.⁶²

Traders and Images of Slaves," *Annals of the New York Academy of Science*, 97 (1977): 286–92; and Gill Shepherd, "The Comorians and the East African Slave Trade," in Paul E. Lovejoy, ed., *The Ideology of Slavery in Africa* (Beverly Hills, 1981), 4–99.

⁵⁹ Mzee Skanda, taped interview with Badawy, Lamu; Salim Kheri, personal interviews, Lamu; and Ylvisaker, *Lamu in the Nineteenth Century*, 154–56.

⁶⁰ Ahmed Jahadhmy, personal interviews, Lamu; and A. A. Skanda, personal interviews with Jahadhmy, Lamu. Leigh cited similar laws for Zanzibar; Kirkman, "The Zanzibar Diary of John Studdy Leigh, Part I," 310. For slave rights in Lamu after abolition, see Strobel, *Muslim Women in Mombasa*, 52–53.

⁶¹ Mzee Skanda, taped interview with Badawy, Lamu; exslave women, personal interviews, Lamu.

⁶² Information on marriage and divorce rates is contained in the Annual Reports of the District Commissioner, Lamu, 1909–63, KNA. Afro-Arab women, personal interviews, Lamu; Abdalla Salim el Hussein, personal interview, Mombasa; and Strobel, *Muslim Women in Mombasa*, 44, 48. Interestingly enough,

Upper-class arranged marriages for family purposes carried many prohibitions between husbands and wives. Sexual relations between the partners was for procreation, not pleasure. Islamic law proscribed a man from touching any part of his wife's body, which limited contact to actual penetration—and that through a small opening in the wife's Turkish-trouser-like sleeping apparel. A man was required to wear his *leso* to bed. Sexual intercourse with the wife (or wives) usually took place only at night, as the man moved from house to house or room to room, visiting each wife in rotation. With a concubine, sex was possible at any time. Because slaves were “nothing” and property,⁶³ a man could shuttle his concubines off alone somewhere and enjoy sexual relations without inhibition, particularly since, with slave partners, physical contact was not restrained by clothing. Indeed, sex play was usually part of the art of seduction between man and concubine, especially if she initiated it; therefore, “sex was more entertaining for the man and for the concubine.” As special enticement, a slave woman threaded beads on a string, fastened them inside her *kanga* around her bottom, and gently swayed her hips back and forth, calling attention to her beads and her anatomy.⁶⁴ And the concubines generally received “special favors” and sometimes *suria* as rewards for the pleasures they gave.

Every household had its head female slave, usually an older woman who had been raised as a member of the household. Among her many functions was to provide sex education for the master's young daughters as well as for young slave girls. Even so, slave girls learned more about sex and at a younger age than their free counterparts, but neither were uninformed from early childhood. At weddings, where children were present, slave women performed dances and sang songs that were at least suggestive and sometimes explicit. One old song, for example, sung over and over at the *vugo* seems on the surface to be a riddle, but both the audience and the singers always knew its metaphoric meaning:

Don't allow me to pass any narrow roads again,
I have got my own eyes and I can see;
Putting in the left [wrong side], that's why my troubles arise.

The narrator is the husband. His wife has refused him entry into her body (the “narrow road”) by closing her legs. He tells her he can see what he wants, and by “putting in the left” or wrong side, or closing her body to him, she has made trouble for the marriage. Such songs instructed young brides and future brides about how

after the legal abolition of slavery and the decline of concubinage, some exmasters participated in what were called “secret marriages” (temporary).

⁶³ Salim Kheri, personal interviews, Lamu; Afro-Arab and exslave women, personal interviews, Lamu; Abdalla Salim el Hussein, personal interviews, Mombasa; and Mzee Skanda, taped interview with Badawy, Lamu. Strobel discussed the role of *somo* (“female slaves who served as instructors in sexual matters to young girls”) in Mombasa and found that, following emancipation, sex education was relegated to outsiders; *Muslim Women in Mombasa*, 11, 19, 201. Lamu informants, however, all agreed that, after slavery was no longer practiced in Lamu, instruction in sexual matters passed to an aunt or other older female family member. Lamu informants now living in Mombasa and familiar with *somo* there suggested that the practice was of African origin and that it persisted in Mombasa because the culture there was more strongly African-influenced than was Lamu's.

⁶⁴ Abdalla Salim el Hussein, personal interview, Mombasa; Abdul Nassir, personal interview, Lamu, April 1980; and Afro-Arab and exslave women, personal interviews, Lamu. I was given a set of beads of the type the concubines used.

not to behave after marriage. Ironically, slaves in *vugo* were supposed to teach submission, not conflict, in marriage, even though the slave women stood to benefit most from marital conflict.

Leisure activities for town slaves far exceeded those for slaves on the shambas. Shamba slaves occasionally joined their neighbors for festivities and dances. In their free hours, women visited neighboring shambas and gossiped while matting bags and rugs or stitching hats, from whose sale they earned a small profit. Men, too, visited one another to talk or trudged through deep sand to town for a few hours of relaxation in the slave quarters in Langoni.⁶⁵ On special days in the year, both town and shamba slaves celebrated with feasting, song, and dance. These holidays included the two celebrations of Idd—one at the conclusion of Ramadan and one at the beginning of the month of pilgrimage to Mecca, where slaves sometimes accompanied their masters—and feast days during the month-long celebrations of the Prophet Mohammed's birthday. One old exslave remembered the *dandalo*, a special dance of African origin, performed by men and women together on festival days in a special area at the southern end of town.⁶⁶ Usually such dances were performed to music played on instruments made of cow or buffalo horns, which were beaten together in rhythm, on a *siwa*, which was generally a cylindrical horn, and on a *sikweya*, which was a special drum that resonated so loudly that everyone in Lamu town could hear it.⁶⁷

When the liwali entertained important visitors to Lamu, he put on a *baraza* on the seafront at the northern end of town. Male and female informants remembered that slave girls danced in colorful costumes with jasmine flowers in their hair and bright make-up on their faces. Sometimes at such fêtes they were dressed in ornate oriental costumes, which had been kept by the old families for this purpose. Former slaves also recalled dancing before British dignitaries after World War II in remnants of these costumes. Only for such occasions was public dancing sanctioned in the northern end of town. But slave family festivities, like weddings, often included dancing, even dancing processions from the shambas to the masters' homes in the northern section of town.⁶⁸ The *maulidi*—a recitation, in Swahili or Arabic, of events surrounding the life of the Prophet, to the accompaniment of music, usually tambourines⁶⁹—was a form of pseudo-leisure. Although maulidis

⁶⁵ Exslave women, personal interviews, Lamu; and Salim Kheri, personal interviews, Lamu.

⁶⁶ Salim Kheri, personal interviews, Lamu; and Ahmed Jahadhmy, personal interviews, Lamu. Although Lamu informants disagreed on some details, they concurred enough to provide a general picture of the ritual; Ahmed Jahadhmy, personal interviews, Lamu; Ali Abdalla el Maawyi, personal interviews, Lamu; Mohamed el Busaid, personal interview, February 1982, Lamu; and Khadija binta Alafa (who allowed her name to be used; she was a member of the *kingi beni* ["a dance association"] as well as an exslave), personal interviews, Lamu. For a discussion of variations of the *dandalo* elsewhere in East Africa, see T. O. Ranger, *Dance and Society in Eastern Africa* (Berkeley and Los Angeles, 1975), 16–30. Also see Strobel, *Muslim Women in Mombasa*, 157–81; and Alan Boyd, "The Musical Instruments of Lamu," *Kenya Past and Present*, 9 (1978): 3–8.

⁶⁷ Exslave women (including Khadija binta Arafa), personal interviews, Lamu.

⁶⁸ Exslave and Afro-Arab women, personal interviews, Lamu; and Ahmed Jahadhmy, personal interviews, Lamu.

⁶⁹ El Zein claimed that no musical instruments were played to accompany a maulidi until Habib Saleh arrived in the late nineteenth century; Habib Saleh, evidently to help alleviate the rigid distinctions with regard to social class, defied the old families by bringing instruments into the mosques. Both Ahmed Jahadhmy and Abdalla Salim el Hussein deny that Habib Saleh was the first to introduce music in Lamu mosques; personal interviews. Lamu and Mombasa.

had a solemn religious function, they provided an air of festivity for slave and free-born alike.

When a household slave married, the master or his son made the arrangements and usually bestowed suitable presents—a set of clothing for the bride or a new *leso* for the groom's wedding night.⁷⁰ Otherwise, the same two items of clothing per year were supplied to household slaves. A household slave had better opportunities than a shamba slave to earn independent income, and a household slave could buy cloth as well as locally made perfumes, extracted from the jasmine flowers. A female slave also used make-up, such as kohl for her eyes and henna, with which, in Arab fashion, she painted designs on her feet and hands, as her mistress did. Dusting the face with flour (as a form of powder) was also commonly practiced by slave and free women. The wives of wealthy town slaves also dressed several cuts above ordinary domestic slaves and even adopted the free-born manner of going about under a *shiraa* carried by her own slaves.⁷¹

Female slaves usually made morning trips to the market and to the fishing boats to purchase the needs for the day. They first had to shop for the mistress, who never went out of the house until late afternoon (and only then to visit family members or attend a *maulidi* in the seclusion of another woman's house). Female slaves tended the children of the household, including their own. They drew water for their mistresses' baths, tended them when they were ill, served all meals, and performed special dances within the harem to entertain them. They ate the same foods as their owners: fish, meat, rice, vegetables, and fruits. But they ate the leftovers—and they ate last. Informants reported that, since the slaves controlled so much of the kitchen work, they could ensure that enough was left over for themselves.⁷² Although upper-class men supervised the work of male slaves, they saw little of the slaves; but upper-class women were almost constantly in the company of female slaves. One aristocratic informant, in her mid-forties in 1980, remembered her mother sitting for hours, stiffly erect, on a special Lamu chair of ebony inlaid with ivory and gold, issuing strict instructions, pointing her finger to emphasize her commands, and sending her slaves scurrying in different directions on their various tasks.⁷³

Although informants expressed different opinions about how well women

⁷⁰ Ahmed Jahadhmy, personal interviews, Lamu; A. A. Skanda, personal interviews with Jahadhmy, Lamu; and Sharif Hasan Badawy, personal interview, March 1980, Lamu. Berg discussed a turban fee that was charged to the slaves in Mombasa; "Mombasa under the Busaidi Sultanate," 175. Cooper said the fee was entirely symbolic, had nothing to do with a turban for slaves; Cooper to author, November 1981.

⁷¹ Afro-Arab women, personal interviews, Lamu; and A. A. Skanda, personal interviews with Jahadhmy, Lamu. Among the many women I interviewed was one old concubine, turned *suria*, who had bright orange medicine around her eyes on the day I spoke with her. She had come to Lamu as a slave for a member of the Busaidi family; on the death of her mistress, her master had married her and, she claimed, remained faithful to her until his death. In addition to providing information on slave dress and make-up, she embodied a Lamu *suria*'s upward mobility. After her marriage, she was openly accepted by Lamu's Afro-Arab women, who believed she was one of them and treated her accordingly. She was invited to their homes, and they visited hers; and she became a leader among the Afro-Arab women because of the high status of her husband. Exslave, Afro-Arab woman, personal interview, February 1980.

⁷² Afro-Arab women, personal interviews, Lamu; Ahmed Jahadhmy, personal interviews, Lamu; and Abdalla Salim el Hussein, personal interview, Mombasa.

⁷³ The woman informant in this instance was the daughter of a concubine, who also married into one of the old, respected families in Lamu, and her comments on her mother's behavior confirm the upward mobility of a *suria* in Lamu society; Afro-Arab woman, personal interview, January 1980.

treated their slaves, most informants believed the women treated slaves kindly, especially those born into the household. As Igor Kopytoff and Suzanne Mier suggested in their work on slavery in Africa, female slaves long attached to a family, as was generally the case in Lamu, were viewed as kin. For this reason, many stayed on after emancipation to live out to the end the roles familiar to them. Even in Lamu today former slaves of the family perform special services for their former mistresses and expect to participate in rituals previously reserved for slaves. At emancipation, in Lamu as elsewhere (so Kopytoff and Meier found), the first-generation slaves were those who left to seek freedom and fortune farther south, in Malindi or Mombasa. Once there, many former female slaves preferred to become prostitutes rather than to continue in domestic service.⁷⁴

A few slave-owning women were, however, capable of extreme cruelty. Consider, for example, an incident recorded by a British district officer:

Until quite recently an old woman used to perambulate the streets of Lamu begging. She had no eyes, nose, or features and no fingers or toes and her body was a mass of scars. I once enquired what disease she had, but was told that years ago she had taken liberties with her mistress, who had thrown her on the fire and held her there.⁷⁵

Whether this type of treatment was atypical we shall never know. Those slaves who stayed on were either old, unhealthy, or had ties to their owners, like many of my informants. Those who disliked their circumstances and could leave, did so. Most fisherman and artisans, for example, seem to have left for paying jobs elsewhere.

According to Lamu sources, many Afro-Arab families accepted British compensation for their slaves but continued the old relationship right under the noses of the British administration.⁷⁶ In a peculiar twist of inheritance laws in Lamu, the unmanumitted slave with an income and savings had no rights to will his property to his family. According to Lamu law on slave rights and obligations, the master or the master's heirs inherited all property from their slaves' estates, leaving the slave families impoverished. This practice continued after the legal abolition of slavery, with masters claiming the estates of deceased slaves as late as the 1920s, perhaps because the slaves had remained with their masters after their emancipation.⁷⁷ Koranic teaching on slavery was enforced by the sharifs, who owned slaves until quite recently. Many who elected to remain eventually joined their masters in idleness. A few at least became shamba owners and began a gradual ascent up the economic, if not the social, ladder.⁷⁸ The old and unhealthy were mostly pensioned off by the British.⁷⁹

⁷⁴ Meier and Kopytoff, eds., *Slavery in Africa* (Madison, Wisc., 1977), Introduction, 73–74; Afro-Arab and exslave women, personal interviews, Lamu; and Arthur Hardinge, FO 107/43, as quoted in R. W. Beachey, *The Slave Trade of Eastern Africa* (London, 1976), 244. Also see Strobel, *Muslim Women in Mombasa*, 138–47. Ali A. Jahadhmy stated that Lamu women went into prostitution after emancipation, but he claimed that most of these women came not from Lamu town but from the neighboring Bajuni Islands; Jahadhmy, personal interview, Mombasa, June 1980. Also see Bujra, "Production, Property, Prostitution," 13–39.

⁷⁵ Unnamed British district officer, as quoted in Berg, "Mombasa under the Busaidi Sultanate," 169.

⁷⁶ Hassan Badawy, personal interview, Lamu; and Afro-Arab women, personal interviews, Lamu.

⁷⁷ Handing over Report, British district officer to his replacement, 1923, Lamu, KNA.

⁷⁸ For a detailed study of the lives of former slaves after abolition, see Cooper, *From Slaves to Squatters*.

⁷⁹ Annual Reports of the District Commissioner, Lamu, 1909, 1915, 1916, 1917, 1918, 1921, 1922, KNA.

SLAVES WHO STAYED ON AFTER EMANCIPATION suffered the fall of the island's fortunes along with their masters. John Clive, British district officer in Lamu, best summarized the eclipse of Lamu when he wrote that after emancipation "the Arab masters were as dependent on their slaves as slaves on their masters."⁸⁰ Or, as one exslave who stayed on Lamu, succinctly put it, "After slavery ended, there were no jobs. I was poor."⁸¹

Today in Lamu reside countless descendants of slaves. Most still live in Langoni or in extensions of the town into the shamba areas. Relations between former owners and exslaves have changed little. The remaining fishermen are still poor, the shambas are all but deserted, and those who formerly tilled the soil now work as porters, carrying heavy loads on the Lamu waterfront or pulling and pushing handcarts with gigantic loads through the streets of town. Most remain illiterate and ignorant, even about religion. A few—mostly men—learn the Koran by rote. Social interaction between the men is as stratified as it was during slavery, except that now all mix freely in the mosques. Among the women some barriers have broken down, but each knows her position in the social hierarchy of the town. Exslaves who continued in the service of the upper class until quite recently still vie for the opportunity to perform tasks formerly assigned to their mothers (and themselves), such as "acting" as head slave at wedding ceremonies. Many of the old elite have survived the economic decline and are again prosperous. The old patronage and support for their former slaves continues, but in modified form. Former masters still give alms to their exslaves, when they are too old or too ill to work. Former slaves and their descendants come to exmasters in times of crisis, like family illness or death. In February 1982 an elder of the Busaid family attended the funeral of one of his exslaves in Langoni, missing a funeral held at the same time for an upper-class resident of the northern end of town.⁸² Repeatedly during my field visits to upper-class women, former slaves came to call on their former mistresses and invariably took a subservient position: the ties of old relationships remain. And exslave women still serve one of their traditional functions—carrying gossip to the women who observe purdah.

Free-born persons and exslaves alike testify that slavery was more tolerable on Lamu than on Zanzibar and the mainland, but this does not, of course, explain why it was tolerable at all to the workers in the households and shambas of the wealthy on that island. Nor can the continued existence of slavery there for almost a century after emancipation be explained by the ignorance of the slaves about their rights under civil law. We can only conclude that the force of tradition, desire for security, and fear of the unknown and untried tended to keep the slaves at their places of work and sustenance until recent times. Undoubtedly, the comparative benevolence of the system—personal treatment, working conditions, adequate diet, and

⁸⁰ Clive, "A Short History of Lamu," 13. Also see A. I. Salim, *Swahili-Speaking Peoples of Kenya's Coast* (Nairobi, 1973), 110–11. Salim, too, believed that Lamu's prosperity ended with abolition. Meryn W. H. Beach also pointed out that many slaves chose to stay with their masters; Beach, "Slavery on the East Coast of Africa," *Journal of the African Society*, 15 (1916): 145–49.

⁸¹ Salim Kheri, personal interviews, Lamu.

⁸² Mohamed el Busaid, personal interview, Lamu.

proximity of master and slave—played their part. That the population remained small and relatively stable in size may also have contributed to Lamu's social stability. The island was not threatened with overpopulation. But most significant of all was probably religion—that is, the continuing belief held by both master and slave that their relationship was sanctioned by God and the Prophet Mohammed and was spelled out in the Koran. That the civil authorities all the way to London tolerated this open secret indicates, furthermore, that they valued order and peace more than they did social reform and obedience to the civil law.



Forms of Resistance: Songs and Perceptions of Power in Colonial Mozambique

LEROY VAIL
and
LANDEG WHITE

"Do you know, my son," Papa spoke ponderously, and gesticulated a long time before every word. "The most difficult thing to bear is that feeling of complete emptiness . . . and one suffers very much . . . , very very very much. One grows with so much bottled up inside, but afterwards it is difficult to scream, you know. . . ."

Mama was going to object, but Papa clutched her shoulder firmly. "It's nothing, mother, but, you know, our son believes that people don't mount wild horses, and that they only make use of the hungry docile ones. Yet when a horse goes wild it gets shot down, and it's all finished. But tame horses die every day—as long as they can stand on their feet."¹

THESE WORDS ARE FROM Bernado Honwana's "Papa, Snake, and I." That story describes with warmth and great subtlety the effects on a small boy of growing up in colonial Mozambique with the feeling, and then suddenly the clear knowledge, that his father is a weak, exploited man who does nothing to resist his exploitation. The setting, as for most of Honwana's stories, is one of those small administrative towns—with a church, a school, a *cantina*, a police post, a club, and an administrative building—that can be found throughout Mozambique. The father works as a minor civil servant in the Portuguese colonial administration, and the crisis in the story comes when the boy hears his father cursing under his breath a white Portuguese neighbor and demands of him, "Why didn't you say that to his face?" At the end of a long evening of tension, father and son come to understand one another. The father realizes that the time has come to admit his son to that adult world of powerlessness and humiliation from which he has so long sought to

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¹ Honwana, *We Killed Mangy Dog and Other Mozambican Stories* (London, 1969), 47–48.

protect him, while the son, suddenly matured, accepts the responsibility of sharing his father's burden. The excerpt quoted above defines a kind of struggle in which the "tame horses" who "die every day" try to bring up their children with the knowledge of values other than those prevailing in colonial Mozambique.

THE HISTORIOGRAPHY OF AFRICA during the 1960s and into the 1970s was dominated by studies of African resistance to European imperialism and to colonial rule. The rise of mass nationalism in postwar Africa led historians to ransack the past for earlier leaders who might have served as role models for the anticolonial struggle, and resistance became nationalism's historical dimension.² African leaders who had attacked colonial armies were granted heroic stature, and Africans who had sought education and had attempted to foster modernization and Westernization were seen as shining apostles of African advancement. In this approach, unexceptional people, ordinary villagers were usually ignored. A book of cardinal importance in shaping this attitude among both scholars and African intellectuals was George Shepperson's path-breaking *Independent African*, published in 1958. In it, the Malawian peasants who failed to support the abortive revolt of the author's hero, John Chilembwe, in 1915 are brusquely dismissed as "a violent vacillating lumpen-proletariat."³

At about the same time that nationalist historians were demonstrating the importance of resistance movements, students of imperialism also recognized the role of resistance in giving shape to imperial expansion. Imperialism and colonialism could not be understood without linking them to the specificities of African societies, and Ronald Robinson and John Gallagher even suggested that the thrust of expansion was called forth by "African proto-nationalism" rather than propelled by any conditions within the European economy.⁴ Common to both approaches was the tendency to concentrate on centralized political structures and to examine the histories of these movements by focusing on the biographies of their leaders.⁵ Where genuinely popular uprisings existed in the historical record, they were usually given standing by the discovery that their effective leaders were scions of "royal" houses. Counterpoised to the resisters in these early studies were those Africans who sought accommodation with the agents of capitalism and colonialism, the "collaborators." These, too, had a modern relevance: if nationalist historiography required the creation of a long line of heroic resisters, the disasters of the first years of independence could usefully be explained in terms of an equally long line of guilty collaborators. Again, however, the collaborators tended to be exceptional

² T. O. Ranger, *Revolt in Southern Rhodesia, 1896-7: A Study in African Resistance* (London, 1967), and "Connexions between 'Primary Resistance' Movements and Modern Mass Nationalism in East and Central Africa," *Journal of African History*, 9 (1968): 437-54, 631-42.

³ Shepperson and T. Price, *Independent African* (Edinburgh, 1958), 402.

⁴ Robinson and Gallagher, "The Partition of Africa," in F. H. Hinsley, ed., *The New Cambridge Modern History*, 11 (Cambridge, 1962): 620. Robinson and Gallagher further developed these notions in their classic *Africa and the Victorians: The Official Mind of Imperialism* (London, 1963).

⁵ T. C. Weiskel, "Changing Perspectives on African Resistance Movements and the Case of the Baule Peoples," in R. E. Dumett and B. K. Swartz, eds., *West African Cultural Dynamics: Archeological and Historical Perspectives* (The Hague, 1980), 550-51.

people—chiefs, mercenary soldiers, educated clerks—rather than ordinary villagers.

By the end of the 1960s historians accepted that “resister” and “collaborator” were clumsy categories with little analytical power. Resistance and collaboration were recognized as alternative strategies open to African leaders as they sought to maintain or augment their power in the face of the colonial challenge. With this recognition came a greater sophistication in dealing with the politics of imperialism and colonialism.⁶ Yet, despite their growing sophistication, resistance studies remain essentially concerned with the military and political history of members of the elite of relatively centralized political structures.

The shift in the mid-1970s away from the search for the roots of nationalism to the search for the roots of underdevelopment, as historians sought to account for the emergence of so many poverty-stricken and politically unstable countries, necessitated a change in the study of African reactions to colonialism. In examining, as the title of Walter Rodney’s immensely influential book succinctly phrased it, *How Europe Underdeveloped Africa* (1972), there has been little scope for discussing African initiatives. The modernizing and Westernizing leaders of the earlier pantheon have become in this new historiography tarnished and irrelevant. But the peasantry, once viewed as obscurantist and backward, has been elevated to an “authentic” stature, largely because of concerns within African countries themselves. In many parts of Africa the peasants in the past showed quick willingness to produce for the market. An influential group of specialists on eastern and southern Africa, defining peasants in terms of their exchange relations, charted the destruction of this efficient and market-oriented peasantry by the forces of colonial capitalism, a process assumed to have been completed by the 1920s.⁷ Even as their work was being published, however, nationalist movements claiming a peasant ideology and mass peasant support were sweeping to power in Mozambique, Angola, and Zimbabwe. The new “political” peasants, figures linked charismatically with the revolutionary peasants of Vietnam and Latin America, have been superimposed on the old “economic” peasants to create, in African countries where agricultural production is faltering and where the failures of the early nationalists are all too apparent, mass heroes of irresistible appeal.

Yet, in the process of elevating the peasantry to its new legitimacy, the vocabulary of the earlier historiography has not been abandoned. Since the early 1970s, resistance studies have been extended to include everything from foot-dragging and dissimulation to social banditry, arson, poaching, theft, avoidance of conscription, desertion, migration, and riot. The new resistance is, in short, any activity that helps frustrate the operations of capitalism or of capitalism’s creations. With the argument moved from the rather uncertain political terrain of the 1960s to firmer economic grounds, much more room is allowed than hitherto for the variety of actual human behavior, which must in itself be a welcome advance. But there are

⁶ *Ibid.*, 550.

⁷ Colin Bundy, “The Emergence and Decline of a South African Peasantry,” *African Affairs*, 71 (1972): 369–88, and *The Rise and Fall of the South African Peasantry* (London, 1979); and Robin Palmer and Neil Parsons, eds., *The Roots of Rural Poverty in Central and Southern Africa* (London, 1977).

problems. Quite apart from the violence done to language by describing such things as desertion and emigration as “resistance,” the continued use of “resistance” and “collaboration” blurs analysis. Is alienation resistance? Is it collaboration to be involved in producing goods for sale on a capitalist market? What happens when tax evasion, desertion, and low worker productivity are practiced by the peasantry in new revolutionary states, and such behavior has to be returned once more to the categories of ordinary human vice? Is it simply a matter of whether the Left or the Right is in power?

Even more worrying is the naiveté of so much resistance psychology, with its accompanying political moralizing. Human reactions to exploitation are enormously diverse. The moment we go beyond the kinds of organized resistance that are politically or militarily visible and try to deduce from people’s behavior their attitudes, perceptions, and cultural values, we find ourselves in areas where terms like “resistance,” “collaboration,” “subjective consciousness,” “false consciousness,” and the like lack the necessary nuance. This is particularly true when dealing with preindustrial societies, for, as George Rudé has cogently remarked, “Such terms as ‘true’ or ‘false’ consciousness [that Marx had originally applied to the industrial working class] can have no relevance at all.”⁸ A frustrated farm laborer in colonial Zimbabwe who misses work because of drunkenness may be viewed as undermining settler capitalism and, hence, as “resisting”; a man who works hard, saves his money to educate his children, and finds his satisfactions in ferocious church-going may well be seen as selling out to the system and, hence, as “collaborating.” But with reference to which theories of the human mind do these labels become the “right” things to be said about such behavior? Or, equally important, the most interesting? Any competent novelist would have no difficulty in devising twenty different narratives describing twenty different ways of coping with exploitation, and each of the twenty heroes and heroines—their days made up of a mixture of semi-resistance and semi-collaboration—would have an equal claim on our attention. Honwana’s “tame horse,” the boy narrator’s father, is presented with great sympathy and respect; the fiction would not be “truer” for being angled as an attack on his “consciousness.”

At the heart of this problem is a difficulty faced with special acuteness by social historians—namely, the difficulty of transcending the particular. Out of all the different case histories, all the variety of human experience, how can one establish historical themes? One solution is to allow available printed sources to operate as a kind of sieve, so that we examine not actual human behavior but behavior that has reached the attention of newspapers, law courts, governmental and nongovernmental reports, and the like. The information so gleaned is more easily quantifiable since processes of selection have already occurred. Attitudes, motives, and cultural values can then be supplied either by reference to anecdotes or by extrapolation from the historian’s own interests and social concerns. A second possibility is to observe, as anthropologists and political scientists have done, institutions such as chiefdom, kinship, the law, religious practices, and similar “objective” phenomena and to deduce from them the nature of ideas, beliefs, and motives. At best, the

⁸ Rudé, *Ideology and Popular Protest* (New York, 1980), 9.

conclusions are interesting and suggestive, but, just as a skeleton is not a living body, so the structures postulated from a study of institutions do not represent the living reality of African thought. At worst, this approach can amount to taking refuge in theory, accepting a version of society and history as correct and then noting selectively whatever fits the theory. Rarely has it been possible to demonstrate in any depth the part played by African perceptions in the unfolding of events. With the thoughts of the actors largely unregarded, the way has been left open for the elaboration of grand theories that lack human and material specificity and that treat people as objects while documenting their exploitation.

A third possibility, and one that is more explicitly "subjective" than the first two, looks at the creations of the people themselves, the forms in which their own major concerns are expressed. Of these, the most accessible are songs and oral narratives, which often, either directly or implicitly, make symbolic statements about common experiences and concerns. Whole groups of people are able to identify with such cultural expressions; hence, these narratives and folksongs reflect popular consciousness. To illustrate such an approach to writing social history "from below," we shall discuss the content of some two hundred African songs recorded in three different areas of Mozambique.

There are difficulties in handling this kind of material for historical purposes. Dating is often difficult, and, even when the poetry is translatable, the words are frequently obscure. One must beware of assuming that every song springs automatically from "the community" or from "the people." Each song is ultimately the work of an individual singer, and, although oral performance (and eventually oral transmission) must depend on some degree of communal acceptance, the link between individual and group opinion has in each case to be demonstrated—either by proving that the song concerned is popular over a wide area or by showing how certain themes and preoccupations keep recurring in large numbers of songs. Ideally, in using such material, one would wish to pay the closest possible attention to the actual meaning of the songs, supported by oral testimony on their meaning to the people who performed and listened to them, and supplemented by investigation into the social position of the singers, into the conventions of the forms they are using, into the context and contingencies of the performances, and into the importance of such songs in the oral literature and general culture of the area as a whole. This is a demanding program, and in the case of some of the material we shall be considering is no longer feasible, leaving us to some extent dependent on interpretive criticism supplemented by intelligent hypothesis.

Set against such difficulties, however, are two enormous advantages. First, many societies in central and southern Africa, as elsewhere on the continent, take for granted that poetry is an appropriate medium for discussing the impact of power. The praise-poem in its many different forms seems to have arisen specifically to provide such a resource, and most of our examples deal in one way or another with the exercise of Portuguese colonial power. Second, it is a convention of the form that power may be openly criticized. Chiefs and headmen may be criticized by their subjects, husbands by their wives, fathers by their sons, employers by their workers, and colonial officials by their underlings in ways that the prevailing etiquettes do not otherwise permit. As one informant declared in speaking of a song directed

against the field manager of a sugar estate, "You could swear at him and he just smiled"; to attack him outside the song "would be just insulting him . . . , just provoking him," but, as long as it is done through singing, "there will be no case."⁹ The form, in short, legitimizes the content: this "free expression" is in many African societies not only tolerated but openly welcomed as a major channel of communication between the powerless and the powerful, the client and the patron, the ruled and the ruler. As a consequence, the kinds of Mozambican songs discussed in this essay provide unparalleled insights into popular perceptions and dissatisfactions during the period of Portuguese colonial rule. As a by-product, these songs make clear how insensitive and inappropriate is the habit of categorizing African reactions in terms of "resistance" or "collaboration."

The songs to be considered are taken from two large groups. The first is a collection of 46 songs that Hugh Tracey compiled in the 1940s, taken from the Chopi musicians of southern Mozambique. As a musicologist, Tracey was attracted to the Chopi by their impressive orchestras of xylophones and, especially, by their spectacular *migodo*, annually staged entertainments made up of dances, songs, and orchestral music in presentations that lasted some fifty minutes each. In his *Chopi Musicians* (1948), Tracey examined seven complete *migodo* (singular, *ngodo*), six from Mozambique and one from the Witwatersrand, which he collected from 1940 to 1943. The second group consists of 187 songs we collected from 1975 to 1977 in the Quelimane district of Mozambique. The majority of these songs date from the 1940s and 1950s, although some are much older, and a few originated in the late nineteenth century.¹⁰

Tracey's *Chopi Musicians* is a minor classic. By paying serious attention to African music, Tracey was a generation ahead of his time, and in the Chopi orchestras he found a splendid subject. Surprisingly, given the infectious enthusiasm of his study, no one has followed Tracey's lead and investigated the *migodo*, either for their content or their performance.¹¹ It was the music of the *migodo*, particularly the instrumentation of the orchestras, that commanded Tracey's most informed attention. Neither as a literary critic nor as a historian was he equipped to plumb the meanings of the words of the songs he transcribed. Although his explanatory notes are frequently very illuminating, his general comments range from the whimsical and patronizing to the completely erroneous. Two examples will suffice to demonstrate the need for a fresh look at his material. The first of Tracey's songs, the fourth section of the *ngodo* by Katini of Wani Zavala's village in 1940, runs as follows:

It is time to pay taxes to the Portuguese,
The Portuguese who eats eggs
And chicken!!
Change that English pound!¹²

⁹ Leroy Vail and Landeg White, "Plantation Protest: The History of a Mozambican Song," *Journal of Southern African Studies*, 5 (1978): 23.

¹⁰ The songs from the 1940s and 1950s are not, in general, still sung, but songsters recalled them to mind at our request.

¹¹ Andrew Tracey and Gei Zantzing filmed two *migodo* in the mid-1970s and published a companion volume to the two films: *A Companion to the Films "Ngodo wa Mbanguzi" and "Ngodo wa Mkandeni"* (n.p., 1976).

¹² Tracey, *Chopi Musicians* (London, 1948), 10. Tracey provided both the Chopi versions of these songs and his own translations.

Tracey commented that the tax question “is an oft-recurring theme in native songs,” and his explanation of the fourth line, that migrant workers returning from South Africa resented having to change their sterling currency to Portuguese escudos at the border with a 10 percent discounting, is helpful. On the song’s central eggs-and-chicken metaphor, however, he had only this to say:

The rightness of reference to the Portuguese as those who eat eggs and chicken will be wistfully admitted by every good Portuguese housewife in the district, of whom there may be a dozen or so. (The last census shows a total of 27 Europeans in the Zavala district.) Had they added fish from the lakes as well, the picture of the available choice of proteins would have been complete! But with the memory of my hostess vividly in mind I would gladly return any day to sample again the hundred and one delicious disguises she has conjured up for this culinary trinity.¹³

There is more in the same vein, and, although the song itself is almost forgotten, Tracey inadvertently helped confirm its central complaint—which is, of course, that the Portuguese consume everything (eggs *and* chicken) without regard for the future. Equally innocent is Tracey’s comment on a song that attacks the use of the brutal *palmatoria*, a perforated paddle used to beat people: “Here is a mystery, the Portuguese beat us on the hands, / Both us and our wives!”¹⁴ Tracey’s “mystery” is a weak word for what ought to be translated as “arbitrary and inexplicable,” and his comment that this is a “very light-hearted lyric” arising from the opinion of the Chopi that they instead should be beaten on “the part specially fattened by nature for sacrifice” is typical of his general refusal to take the words of these songs seriously. There are real grounds, then, for re-examining his material, partly to illumine the realities of Chopi society and partly to provide comparative material for the larger group of songs that we collected.

MOZAMBIQUE’S QUELIMANE DISTRICT, which is bounded by the Zambesi, the Shire, and the Ligonha rivers, has for the greater part of the twentieth century operated as an enclave economy.¹⁵ From the 1890s to 1930, most of the district was under the quasi-governmental control of four large plantation companies. Sena Sugar Estates, founded with British capital and under largely British management, controlled three *prazos* (estates leased from the Portuguese crown) straddling the Zambesi River. The Companhia do Boror, founded with French capital, grew coconuts and sisal in a group of five *prazos* to the north and northwest of the town of Quelimane. The Société du Madal, based in Monaco, ran coconut plantations in four *prazos* to the south of Quelimane. Finally, the Companhia da Zambesia, the only Portuguese firm and based in Lisbon, grew coconuts and sisal in three separate *prazos*. Two further companies founded with Portuguese capital operated briefly until each was absorbed by the expanding Sena Sugar. The Companhia do Luabo grew coconuts and sugar along the Zambesi, while the Empresa Agrícola de Lugella controlled three huge *prazos* bordering Nyasaland. These companies ran most of Quelimane district until 1930, when Salazar abolished the concession system. Administered from their head offices in London, Paris, Monaco, and Lisbon, these firms levied

¹³ Tracey, *Chopi Musicians*, 10–11.

¹⁴ *Ibid.*, 15.

¹⁵ For a detailed account of the history of this area from the early 1800s to 1975, see Leroy Vail and Landeg White, *Capitalism and Colonialism in Mozambique: A Study of Quelimane District* (Minneapolis, 1980).

labor, collected taxes and customs' revenues, coined money, monopolized trade, ran their own police forces, and distributed justice throughout their *prazos*—limited only by Portuguese “agents of authority,” whose salaries they paid. The sole exceptions to this regime in the whole district were the circumscriptions of Maganja da Costa and of Alto and Baixo Moloque, which remained under the control of the state and which after 1903 became suppliers of labor to the Witwatersrand.

After 1930, when state administration was extended to the whole district, Quelimane remained a single and self-contained economic unit. Although Salazar's “reforms” were meant to “Portugalize” the district's economy, the new administration's most pressing economic problem was to supply workers not only to the established plantation companies but also to new tea-producers and to the concession holders who had exclusive control over the compulsory commodity production of rice and cotton. At first, the state employed European labor organizers, but, as the demand for workers grew, it decided to appoint African chiefs, who were paid to act as front men for the government, the plantations, and the concession holders. Legally, all Africans were required to pay a head tax, which corresponded to the minimum wage for three-to-four months' work, and could be obtained from any source. In practice, especially after the Governor-General's Circular of October 1942, all male Africans between the ages of eighteen and fifty-five were obliged to prove that they had worked for at least six months of each year as laborers in the service of the state, private concession holders, or companies. In practice, too, labor on the cotton and rice concessions was supplied by women. No one legally left Quelimane district to work for the higher wages available elsewhere. Apart from a certain degree of temporary clandestine emigration to Manica-Sofala, to Southern Rhodesia, and to South Africa, and apart, too, from a substantial degree of permanent emigration to Nyasaland, which placed constraints on labor recruitment in border areas, Quelimane district was successfully administered as a labor reserve for the plantation companies and for the concession holders. Although the state constantly modified its labor strategies in the face of African reactions, not until April 1974 was the essential pattern of the Quelimane labor reserve broken.

Behind the apparent uniformity of these pressures on the African population, however, were two quite distinct historical experiences. When the plantation companies gained control of their *prazos* between 1890 and 1904, they acquired estates that had in most instances been in existence since the seventeenth century. Despite the repeated efforts of Lisbon governments from 1832 onward to reform the system of land tenure in the Zambesi valley, the *prazo* system had proved extraordinarily tenacious. This was principally because—as the wars of the 1850s and 1860s made clear—no Lisbon government could control Zambesia except in alliance with the *prazo* holders, whose interests were diametrically opposed to metropolitan liberal policies and whose power allowed them to ignore decrees and laws, to withhold troops from the government, and even to talk of secession. Only when international pressures, culminating in the British Ultimatum of 1890, forced Portugal to appear to be developing its colonies instead of just occupying them were the earlier attempts to abolish the *prazo* system abandoned. Instead, the Portuguese

modified the system to attract international capital, offering longer-term investments and more stringent labor laws. At that point, the major plantation companies moved in. But, in taking over the *prazos*, they became heirs to that relationship between *prazo* holder and African cultivator, between landholder and tenant, between patron and client that had characterized the system since the late seventeenth century.

The African inhabitants of the *prazos* were accustomed to a system of administration that had as its ultimate head the *prazo* holder, and they were used to paying taxes to the holder, both in money and in produce. They were even used to supplying tribute labor. But they were also accustomed to a pattern of reciprocity that allied their chiefs with the *prazo* holders (if they were peasants) or that gave them the semi-protected status of one of the many categories of slaves. The *prazo* holder was expected to provide military protection and to offer help in time of famine. Against any *prazo* holder who exceeded his rights or failed in his obligations, the Africans applied the ultimate sanction of emigration. This rough *modus vivendi* was drastically modified and curtailed in scope during the long upheaval of the nineteenth-century slave trade. But it remained in the culture as a set of perceptions about the rights and duties of government, waiting to be employed by the workers and peasants against their new company overlords.¹⁶

This *modus vivendi* applied, however, only in those parts of the district that had historically been incorporated into the *prazo* system—that is, in the area south and west of Quelimane and along the banks of the Zambesi River. When troops of the Companhia da Zambesia invaded the newly designated *prazos* of Milange, Lomwe, and Lugella in 1899, they came as an occupying colonial army into territory that had been completely independent. None of the company's demands—for the head tax, corvée labor, or a monopoly of commerce—could be comprehended as an adaptation of earlier systems of government. The reaction of the people of the area was, first, to attempt armed resistance and, when that was suppressed, to flee to coastal areas that the state had yet to bring under control, to fortified hilltop villages that had been their refuge during the slave trade, and in great numbers to British Central Africa across the border. Similar problems, though on a scale modified by the different terrain and by the greater distance of the border, were encountered by Sena Sugar and by the Companhia do Boror, which tried to impose their demands on inland areas, and by the state, which attempted to levy the head tax and to conscript migrant labor in the new circumscriptions of Maganja da Costa and Alto and Baixo Moloque. In none of these areas was it possible to imitate the kind of rule that had evolved in the older *prazos*, where by 1914 there had already emerged a version of company paternalism whose ultimate source was the idea held by the people themselves of what the *prazo* system was all about: the reciprocity of patron and client.

Instead, after decades in which migrant labor was obtained for the sugar, copra, sisal, and tea plantations by the old-fashioned methods of licensed raiding and in which mass emigration to Nyasaland continued unabated, Portuguese control of

¹⁶ *Ibid.*, esp. chaps. 4, 8.

the interior of Quelimane district was achieved after 1945 through the agency of state-appointed chiefs and headmen. "The chief was chosen by the government. When they came in a village like this one, they would gather everybody together and look for a clever one and name him a chief, and everybody would have to respect him for his title. Even a bad one."¹⁷ These chiefs became the administration's agents in tax collection, labor recruitment, and the enforcement of schemes for the compulsory production of rice and cotton. Though liable to ill treatment and dismissal if they failed to cooperate, the chiefs were paid handsomely and were hated by their new "subjects," for whom they epitomized colonial rule. Thus, although the whole of Quelimane district operated as a single labor pool for the plantation companies, African reactions to their labor exploitation were far from uniform. The two extremes of response are represented by the Lomwe-Chuabo-speaking peoples of the interior highlands and by the Sena-Podzo-speaking peoples of the north bank of the lower Zambesi around Sena Sugar's head offices, first at Mopeia and then at Luabo. Elsewhere in the district, the distinctions were less clear cut; local circumstances and Portuguese pressures produced differing results.¹⁸

The Lomwe-Chuabo songs date mainly from the 1940s to 1960s, and they are dominated by two themes linked by a central preoccupation. The first and most obvious of these themes, expressed in more songs than we are able to quote here, is a bitter hatred of the state-appointed chiefs and headmen. Such hatred arose partly from the chiefs' official function, that of labor recruiter, responsible for supplying labor to the company recruiters or to the police, or for presenting the requisite number of workers at the administration building or at the roadside recruiting posts.

The headman - ay - ay - ay - ay
 The headman harassed and seized one of my sons for Luabo,
 The other went to São Thomé and never returned.
 I'm going to bury the headman and build my house on his head!
 The headman - ay - ay - ay - ay!¹⁹

Similarly, the chiefs and headmen were hated for their role as tax collectors.

I'm being tied up
I'm being tied up far from home
 Tax, tax,
*My heart is angry.*²⁰

They were hated for their role in the compulsory cotton- and rice-growing campaigns. One of the women's songs derives from the Macuse division of the Azenha, Barbosa, e Mendes rice concession:

¹⁷ Group Interview, Muanavina Compound village, Luabo, Quelimane district, August 26, 1975. (All interviews are from Quelimane district unless otherwise specified.) ¹⁸ Vail and White, *Capitalism and Colonialism in Mozambique*, esp. 308–14.

¹⁹ Sung in Chuabo by Janta Lakriman and Luis Prazo of Maganja da Costa, at Checanyama Compound village, Luabo, August 30, 1975. Luabo is one of the largest of the Sena Sugar Company's plantations. For the Chuabo version of this song, see Vail and White, *Capitalism and Colonialism in Mozambique*, 360.

²⁰ Sung in Chuabo by Pedro Rovi and Joao Orfumane of Maganja da Costa, at Checanyama Compound village, Luabo, August 30, 1975. The italicized lines are those that are sung in chorus by the listeners. For the Chuabo version, see Vail and White, *Capitalism and Colonialism in Mozambique*, 361.

Because it's me, I'm being beaten,
 Because it's me, I'm being beaten,
 Because it's me, I'm being beaten,
 Look, friends!

I've been tied up, I've been tied up,
 I've been tied up, I've been tied up,
 I've been tied up, I've been tied up,
 The chief - ay!²¹

They were hated for their sexual abuse of the wives of men they had dispatched as migrant laborers (chiefs were permitted to retain 40 percent of the recruitment tax of twenty-five escudos on workers contracted from their area who traveled *without* their wives). One supremely scornful song satirizes the chiefs under a set of derogatory names: *aNankhollo* ("he-goat"), *aNamthullu* ("impotent bull"), *aMullalleya* ("court messenger"), and *aMukhopella* ("upstart")—all of which indicate the contempt the Lomwe women had for the nonentities appointed by the Portuguese to such positions. The nickname "hoe-handle" refers to their unflagging licentiousness:

aNankhollo, hoe-handle,
 aNankhollo, hoe-handle,
 It's not that I want to summon you!
 You chose my husband yesterday
 You sent him to the work place.
 Then you came banging on my door.
 You took me for a prostitute,
Ali!
 You took me for a prostitute!
Ali!

aNamthullu, hoe-handle,
 aNamthullu, hoe-handle,
 It's not that I want to summon you
 You chose my husband yesterday
 You sent him to the work place.

²¹ Sung in Chuabo by Teresa Sabinu, at Macuse, September 2, 1977. The words in Chuabo are

Kokala miyo kino ovadiwa
 Kokala miyo kino ovadiwa
 Kokala miyo kino ovadiwa
 Ona, siyaya!

Ndimangiwa, ndimangiwa
 Ndimangiwa, ndimangiwa
 Ndimangiwa, ndimangiwa
 Samasoa - ay!

For further discussion of these campaigns, see Leroy Vail and Landeg White, "Tawani, Machembero!: Forced Rice and Cotton Cultivation on the Zambezi, 1935–1960," *Journal of African History*, 19 (1978): 239–63; and Allen Isaacman *et al.*, "'Cotton Is the Mother of Poverty': Peasant Resistance to Forced Cotton Production in Mozambique, 1938–1961," *International Journal of African Historical Studies*, 13 (1980): 581–615.

Then you came banging on my door.
You took me for a whore,

Ali!

You took me for a whore,
Ali!

aMullalleya, hoe-handle,
aMullalleya, hoe-handle,
My heart is worn out.
My heart is worn out!
My husband came back on the day before yesterday.
Today he's picked up again.
I'm worn out!

Ali!

Ay - ay - ay - ay - ay
Ali!

aMukhopella, hoe-handle,
aMukhopella, hoe-handle,
It's not that I want to summon you!
I too would like someone to work in my garden,
Last year, when you picked him up,
You took me for a whore,

Ali!

You took me for a whore!
*Ali!*²²

Above all, they were hated as figures who possessed a type of power that had no local precedent:

You can do it
You can do it
You can do it
You can do it
You can find the chief and beat him up
You can do it
But the chief is a powerful man!²³

Created in accordance with Portuguese notions of what African political relations were, the chiefs and headmen exercised their new authority arbitrarily and despotically.

The second theme of these songs is the situation not in the village under the headmen but at work on the plantations. The Lomwe-Chuabo songs are more sharply critical of the inequalities of the company system than any others from the district:

²² Sung in Lomwe by Armena Muhinayula and Helena Souzinho of Mulevala, Ile, at Checanyama Compound village, Luabo, August 30, 1975. For the Lomwe version of this song, see Vail and White, *Capitalism and Colonialism in Mozambique*, 362.

²³ Sung in Chuabo by Parose Kwiri of Baixo Licungo, at Juncua Compound village, Marromeu, Beira District, September 7, 1975. For the Chuabo version of this song, see Vail and White, *Capitalism and Colonialism in Mozambique*, 360.

I'm working, in hunger,
 The owners are full;
 It's a bad sign.
 It's a bad sign.²¹

And these songs express, more clearly and concisely than any others, the destructive impact of the companies' demands for labor:

This finished off the young men,
 The Company - ay,
 This finished off the young men,
 The Company - ay,
 This finished off the young men,
 Hair,
 This finished off the young men,
 Since they started,
 This finished off our grandfathers,
 Cunha,
 This finished off the young men,
 Cunha,
 This finished off the young men,
 *The Company - ay.*²⁵

The company referred to is Sena Sugar, and the officials mentioned (Hair and Cunha) were plantation managers. A women's song from Macuse makes the same point about labor recruited for the Boror Company's sisal plantations at Namacurra:

Ay - ay - ay e-e ay - ay - ay
 e-e ay - ay - ay
 Listen to me, all of you,
 My children are dying
 My children are dying—hard work for Kokora!²⁶

Kokora, which means "sweeping," was the African nickname for Nelson Saraivo Bravo, administrator at Namacurra. The background to this name and to the complaint expressed in the song is that the sisal plantations, given their location in the hot sandy plain inland from Quelimane and the prickly nature of the plant, were invariably unable to obtain labor except by the direct intervention of the administration's police.

Perhaps the most moving of all these songs, however, is a women's pounding

²¹ Sung in Lomwe by Armando Francisco and Manuel Shipitela of Mulevala, Ile, at Muidi Compound village, Luabo, August 9, 1975. For the Lomwe version of this song, see Vail and White, *Capitalism and Colonialism in Mozambique*, 365.

²⁵ Sung in Lomwe by Armando Francisco and Manuel Shipitela of Mulevala, Ile, at Muidi Compound village, Luabo, August 9, 1975. For the Lomwe version of this song, see Vail and White, *Capitalism and Colonialism in Mozambique*, 365.

²⁶ Sung in Chuabo by Familia Sayidana, at Macuse, September 2, 1977. The words in Chuabo are

Ay - ay - ay e - e Ay - ay - ay
 e - e Ay - ay - ay
 Mundi velele
 Ananga nomala
 Ananga nomala—labwa Kokora!

song from Ile, which compares what is happening at home within the local cotton concession with what is happening 275 kilometers away on Sena Sugar's Luabo estate:

I suffer, I do,
Oyi - ya - e - e
 I suffer, I do,
I suffer, my heart is weeping,
 What's to be done?
I suffer, my heart is weeping,
 I cultivate my cotton,
I suffer, my heart is weeping,
 Picking, picking a whole basketful,
I suffer, my heart is weeping,
 I've taken it to the Boma there,
I suffer, my heart is weeping,
 They've given me five escudos
I suffer, my heart is weeping,
 When I reflect on all this
Oyi - ya - e - e

I suffer, I do,
I suffer, my heart is weeping,
 My husband, that man,
I suffer, my heart is weeping,
 He went there to Luabo,
I suffer, my heart is weeping,
 He went to work, work hard,
I suffer, my heart is weeping,
 He broke off some sugar-cane for himself,
I suffer, my heart is weeping,
 Leaving work, he was arrested,
I suffer, my heart is weeping,
 He was taken to the police,
I suffer, my heart is weeping,
 He was beaten on the hand,
I suffer, my heart is weeping,
 When I reflect on all this,
Oyi - ya - e - e
 I suffer, I do,
*I suffer, my heart is weeping.*²⁷

In its broad details, this song is accurate. The singer received only five escudos for six months' work in the cotton fields. (She did not exaggerate. In 1940 the average annual income in Mulevala, where she came from, was eleven escudos for cotton pickers, and the district was eventually declared unsuitable for cotton.²⁸) Meanwhile, her husband was beaten for eating a piece of the sugar cane he was harvesting. (Again the complaint is justified. Sena Sugar claimed losses of hundreds

²⁷ Sung in Lomwe by Armena Muhinayula, Helena Souzinho, and Casavera Fernando, from Mulevale, Ile, at Checanyama Compound village, Luabo, August 30, 1975. For the Lomwe version of this song, see Vail and White, *Capitalism and Colonialism in Mozambique*, 352–53.

²⁸ Vail and White, *Capitalism and Colonialism in Mozambique*, 316.

of tons of sugar cane per year through theft, and the penalties were severe.²⁹) Implicit in this comparison of what happens at home with what happens on the distant plantation is a comment on the whole unified economic system in which the singer was trapped. But the song's shape, as it moves from the singer's experience to that of her husband, stresses an even more important theme, that of the separation of husband and wife—the fundamental impact of this system of migrant labor on the family.

This concern is, indeed, the most striking feature of the Lomwe-Chuabo songs. Although they characterize more sharply and eloquently than any others from the district the injustice and brutality of the plantation and concession systems, they make no mention at all of Portuguese rule. None of these songs, on internal evidence alone, can be identified as coming specifically from Mozambique. Although the themes of the songs are representative of a fairly wide area of Quelimane district, the actual targets of protest are very local indeed. The songs attacked particular companies and individual chiefs and headmen. There is no larger colonial, or nationalist, or revolutionary, or even ethnic dimension. The central preoccupation is the family—what is happening to husbands, wives, and children and to the village farms. All the anger, grief, suffering, and loneliness of these songs ultimately arise from the destruction of a way of life centered on the rural homestead. One last song, sung by a woman from Maganja da Costa where Lopes e Irmão ran the rice concession, makes the point poignantly and economically. The song begins with the message that her husband has reached Sena Sugar's Marromeu plantation:

Marromeu has spoken
He has arrived;
 Marromeu has spoken
He has arrived;
 Marromeu has spoken
He has arrived, Marromeu.

I grow my rice
He has arrived;
 I am watching the road
He has arrived;
 The road is empty of people
*He has arrived, Marromeu.*³⁰

This concern in the songs is supported by oral testimony:

When people were arrested and taken to the *Posto* and told they were going to work, they used to write their names down, and if one of them ran away, the rest of his family would be arrested—such as his wife and children. So, if you're on a journey, you don't run away because you know your family is going to suffer. The family in the village will suffer.³¹

²⁹ G. Hornung to Pyke, April 15, 1927, enclosure in Pyke to the Foreign Office, April 19, 1927, in Public Record Office, London, Foreign Office, 371/11989.

³⁰ Sung in Chuabo, by Paterina Joao and Palmira Goodbye of Baixo Licungo, at Juncua Compound village, Marromeu, Beira District, September 2, 1975. For the Chuabo version of this song, see Vail and White, *Capitalism and Colonialism in Mozambique*, 354.

³¹ Group Interview, Muнавина village, Luabo, August 26, 1975.

What the Lomwe-Chuabo-speaking peoples desired above all else was to be left alone, to be allowed to continue living and working undisturbed in their small-scale village systems. They carried that desire over into the period of Frelimo's liberation struggle, and it has largely colored and ordered their reaction to Mozambique's independence.³²

The Sena-Podzo songs of the north bank of the lower Zambesi are quite different indeed. For people living in the shadow of the country's largest capitalist company, Sena Sugar Estates, the local chiefs and headmen were insignificant and slightly comic figures. The people of this region had historically been part of the *prazo* system, and the demands of the *prazo* holder for taxes and labor were familiar—even acceptable—so long as they did not exceed certain limits. The taxes paid and labor supplied formed part of the bargain between rulers and ruled. As late as 1945, three years after the circular of 1942 had made six months' labor obligatory, Sena Sugar still required only some three-and-a-half to four months' work from its locally recruited workers to fulfill their tax obligations. Any attempt to go beyond this resulted in widespread absenteeism and emigration, which made the effort to get more than four months' work pointless. For most of this century the company made good its larger requirements with migrant labor, which proved much easier to discipline and control. It thus became a key part of the system that the company obtained at fixed prices from local producers the foodstuffs needed for the labor compounds in which the migrant workers were housed. The exception to this rough and unequal *modus vivendi* between former *prazo* holder and former *prazo* inhabitants was the cotton concession, which Sena Sugar held for the area from 1936 to 1961. Because cotton was a six-month crop and because, to protect its labor supplies, Sena Sugar demanded that women grow the cotton, for twenty-five years the company and its agents presided over a system of oppression at least as bad as the one prevailing in the Lomwe-Chuabo areas further north.³³ By this stage, however, the Sena-Podzo peoples were inclined to put the blame not on the company but on the Portuguese state.

The Sena-Podzo songs are not about chiefs or taxation or the decay of village and family life. These are not perceived as the main issues. Instead, they are about the abuse of power and the breaking of implied agreements between patron and clients. Exploitation is seen in entirely personal terms, and its human faces are identified. A typical song begins with the name of the person who is attacked, often repeated once or twice, followed by a denunciatory epigram that sums up why he or she is especially hated. In the 1950s Fernando Braz Valezim, for example, was an overseer on Sena Sugar's cotton concession:

Varajin - ay
 Ay - ay
 Varajin - ay
 Ay - ay
 Varajin - ay
 *Varajin must go home, Varajin's too much!*³⁴

³² For a full discussion, see Vail and White, *Capitalism and Colonialism in Mozambique*, chap. 9.

³³ *Ibid.*, 272–78, 314–25.

³⁴ Sung in Sena by Nkanda Brassa and the women of Madumo village, Luabo, August 17, 1975. The words in

Muripata was one of the African police attached to the Luabo police post and, like Varajin, had the reputation of confiscating babies to make their mothers work faster:

Muripata
I'm exhausted,
Ay - ay - ay, Ay - ay - ay
I want to put the child on my back, Muripata,
Ay - ay - ay, Ay - ay - ay

Muripata,
I'm falling asleep,
Ay - ay - ay, Ay - ay - ay
I want to put the child on my back, Muripata,
Ay - ay - ay, Ay - ay - ay

Muripata,
Witchcraft doesn't harm him,
Ay - ay - ay, Ay - ay - ay
I want to put the child on my back, Muripata,
Ay - ay - ay, Ay - ay - ay.³⁵

And Dona Anna d'Oliveira, a small planter and trader, was annually provided with laborers by the local *chefe do posto*:

Dona Anna, Ay - ay,
Ay - ay - ay
Ay - ay, Dona Anna,
Ay - ay,
"Go there!" telling us,
"Come here!" telling us.

Sena are

Varajin - ay
Ay - ay
Varajin - ay
Ay - ay
Varajin - ay
Varajin aende kwawo Varajin anyanya.

For another song attacking Varajin, see Vail and White, *Capitalism and Colonialism in Mozambique*, 321–22.

³⁵ Sung in Sena by Julia Manico and the women of Mapangane village, Luabo, August 10, 1975. The words in Sena are

Muripata
Ndina lezera
Ay - ay - ay, Ay - ay - ay
Ndina funa kubala mwana, Muripata
Ay - ay - ay, Ay - ay - ay.

Muripata
Ndina gona
Ay - ay - ay, Ay - ay - ay
Ndina funa kubala mwana, Muripata
Ay - ay - ay, Ay - ay - ay.

Muripata
Sina lozoa
Ay - ay - ay, Ay - ay - ay
Ndina funa kubala mwana, Muripata
Ay - ay - ay, Ay - ay - ay.

Leroy Vail and Landeg White

Dona Anna, Ay - ay,
 Ay - ay - ay
 Ay - ay, We used to be tied up,
 Ay - ay,
 "Go there!" telling us,
 "Come here!" telling us.

Used to be tied up, Ay - ay,
 Ay - ay - ay
 Ay - ay, tied up with rope,
 Ay - ay,
 "Go there!" telling us,
 "Come here!" telling us.

POLICEMAN: Dig, dig, dig, *ari!*
 Beat, beat, beat this person, *ari!* Beat her, beat her!
 (SOBBING NOISES)
 Son of a bitch, you dog, son of a bitch!
 You haven't worked, you lazy so and so.
 I'll fuck you today, I'll fuck you today, I'll fuck you today, *ari!*
 This woman wants to be fucked! I've caught you this time!

WOMAN: No, I don't want to be beaten.
 It's better to be screwed.

POLICEMAN: Son of a bitch!
 Dona Anna, father!
 Ay - ay - ay
 Ay - ay, We used to be tied up, father!
 Ay - ay
 "Go there!" telling us,
 "Come here!" telling us.³⁶

³⁶ Sung in Sena by Julia Manico and the women of Mapangane village, Luabo. August 10, 1975. The words in Sena are

Dona Anna, Ay - ay
 Ay - ay - ay
 Ay - ay, Dona Anna
 Ay - ay
 "Bwera uku!" ntekume
 "Bwera kunu!" ntekume
 Dona Anna, Ay - ay
 Ay - ay - ay
 Ay - ay, ndika mangewa
 Ay - ay
 "Bwera uku!" ntekume
 "Bwera kunu!" ntekume
 Naka mangewa, Ay - ay
 Ay - ay - ay
 Ay - ay, mangewa na nkambala
 Ay - ay
 "Bwera uku!" ntekume
 "Bwera kunu!" ntekume.

In this third song, the actors in the inset drama were both women. One played the part of the policeman sent by Dona Anna to recruit workers, and the other the woman being recruited. The “policeman” chased the “woman” round the circle of singers, shouting and beating her with a truncheon before enacting the rape to screams of laughter from the audience.

Some of the differences between these songs and those from the Lomwe-Chuabo area are already evident. The Sena-Podzo songs are cruder, louder, less decorous, less controlled, more peremptory, and more vigorous, and they are driven by a fierce conviction of protest. The targets are individuals—the cotton and rice overseers, the labor recruiters, the concession holders, the small planters, the field capitaes, the administration police, company officials, and the like. The subject is violence, the state- and company-sanctioned violence that is at the heart of the system. In particular, the subject is violence against women, for all three are women’s songs. There are men’s songs from the cane fields—which share, and probably originated, this same form. Of the following songs, the first is addressed to a Portuguese overseer on Sena Sugar’s Luabo estate:

Oso Romeya - ay
 O - o
 O
 O
 O - o
 O,
 Oso Romeya - ay,
*I peeped and saw his mother's cunt!*³⁷

POLICEMAN: Lima, lima, lima, *ari!*
 Menya, menya, menya muntu! *Ari*, menya menya!
 Fedla puta, mwanamba, fedla puta.
 Nkabilima tai polola tai
 Nakugona lero, nakugona lero, nakugona lero.
Ari nkazi asafuna goniwa uyu. Ndakupata lero.

WOMAN: Nkabi nyo nyo nyo nyo pianga
 Goniwa kwene mbwene.

POLICEMAN: Fedla puta.
 Dona Anna, baba.
 Ay - ay - ay
 Ay - ay, Tika mangewa, baba
 Ay - ay
 “Bwera uku!” *ntekume*,
 “Bwera kuuu!” *ntekume*.

³⁷ Sung in Podzo by Dose Chonze, Jiwa Todo, and the men of Madumo village, Luabo, August 17, 1975. The words in Podzo are

Oso Romeya - ay
 O - o
 O
 O
 O - o
 O
 Oso Romeya - ay
Da nyinda kwiri ya mache damoua.

The second is addressed to an African capitão on the same estate.

Weno,
 It wants to touch you,
 Weno,
 O - ay - ay,
 Weno,
 O - ay - ay,
 Weno,
 *His mother's cunt!*³⁸

The third, however, is an all-purpose insult:

O Capitão,
 O Capitão,
 O Capitão,
 *Your mother's prick, Capitão, I'm tired!*³⁹

When we recorded these songs from retired field workers, they were sung with tremendous relish; the *O - o's* and *Ay - ay's* built up much suppressed laughter before the explosion of the insult. They were obviously very satisfying to sing in the cane fields to overseers who had to put up with them, both because the prevailing etiquette dictated that they should and because the men worked faster when singing rhythmically. All the same, it is hard not to conclude that the Sena-Podzo men's work song, with its conventional and repetitive obscenities, is a somewhat limited form, especially when set along side the Lomwe-Chuabo men's work songs. It leaves us with an impression of broad dissatisfaction with labor conditions on the estates, but it supplies no details. One of the singers of *Oso Romeya* filled in the background. "This is about suffering. While pushing trucks, loaded with sugar cane. And while you push, someone is hitting you on the back. On each truck there used to be four people to push."⁴⁰ The comment is more informative than the song!

Only in the mouths and minds of the Sena-Podzo women did the work song, as danced in the village, breach the cramped confines of the name-plus-epigram format. A typical performance begins with the women standing in a circle, bending forward from the waist and clapping or shaking tin *machacha* as rhythmic accompaniment to the lead singer and the chorus. One at a time, they perform brief solo dances, backing slowly round the circle. After several verses, however, the song breaks off while an extemporized drama is performed, illustrating the song's theme. The stage is the circle of singers, and anyone can perform; the actors are

³⁸ Sung in Podzo by Dose Chonze, Jiwa Todo, and the men of Madumo village, Luabo, August 17, 1975. The words in Podzo are

Weno,
 Ina kuta kupata
 Weno
 O - ay - ay
 Weno
 O - ay - ay
 Weno
 Pa nyini pa mache.

³⁹ Sung in Sena by Jose Kashkinya, Madumo village, Luabo, August 17, 1975. For the Sena version of this song, see Vail and White, *Capitalism and Colonialism in Mozambique*, 363.

⁴⁰ Group Interview, Madumo village, Luabo, August 17, 1975.

frequently replaced part of the way through by women who feel they can do better. The audience consists of the remaining women, who encourage and applaud the caricature of bribery and beatings and rape. Thus, while the men insulted their overseers with uninventive obscenities, the women characterized in detail the whole range of the regime's impact on their lives. In one inset drama, part of a song that consists of the single repeated phrase "They used to tie us up," two policemen from Luabo raid a village in search of illegal *kachasu* stills:

FIRST WOMAN: (WHISPERING) The white man's coming!

SECOND WOMAN: Sh! Sh! *Chefe da Policia!*
Take care of the barrel.

FIRST WOMAN: Who are they? Who are they? Joaqui?
Who sent them?

SECOND WOMAN: He's from the administration. It's Blanket!

FIRST WOMAN: But it's Sunday today.
Our husbands went to look for sugar cane.

SECOND WOMAN: Let's pretend we're digging.

FIRST WOMAN: Now today, should there be inspection today?
Today, we're going to be arrested here.
Run away with the children, with the women,
everybody, run away to the reeds.
The people are going.
It's Rodrigues, here he comes.
We're being tied up.
Five litres, ten litres, we're carrying the
bottles in our hands.
We must sleep in hiding.
Now they're arresting us.

SECOND WOMAN: When we get there, we're going to be beaten up.
We're going to be held by the head, and have our heads
thumped against the cement wall.
Getting beaten, after everything else.
Thump! Thump!¹¹

The policemen in this song are Joaquim Rodrigues and Agostinho Pires, nicknamed "Blanket" because "he used to beat people all over."

The most important, and the most popular, of these Sena-Podzo songs, however, was originally a men's work song before it was "taken to the village" to be performed in expanded versions by both men and women. The *Paiva* addressed in the song was originally José de Paiva Raposo, in whose name the *prazo* on which Sena Sugar first operated was initially leased in the late nineteenth century and who, as *prazo* holder, had the right to levy labor. Other members of the Paiva Raposo family worked for the company until the 1950s. The family's continued association with the company helped keep the song alive by giving each generation of workers a fresh "Paiva" to attack. But *Paiva* is more than the name of a person. It became the local name for the company itself as *prazo* holder, the name under

¹¹ Performed in Sena by Vittoria Camacho and the women of Muanavina village, Luabo, August 24, 1975.

which Sena Sugar had appropriated the land and commandeered the labor. The song, then, is really addressed to the company itself. This version was sung by a male professional singer and composer in a village close to Luabo:

Paiva,
 Paiva, I've felt it
 Paiva,
 Paiva, I've complained
 Paiva,
 Paiva, I've complained
 Paiva,
 The mbira is my witness
 Paiva,
 Paiva, father, I've wept
 Paiva,
 Paiva, going weeping
 Paiva,
 Paiva, going weeping
 Paiva,
 Paiva complained
 Paiva,
 Paiva, I've fallen on a fire
 LUABO!

Paiva,
 Paiva, I've complained
 Paiva,
 Paiva, I'm speechless
 Paiva,
 Paiva, I'm speechless
 Paiva,
 Paiva, I've wept today
 Paiva,
 Paiva, I've complained
 Paiva,
 Paiva, I've fallen on a fire
 Paiva,
 Paiva, going weeping
 Paiva,
 Paiva, I've fallen on a fire
 PAIVA LUABO!

Paiva,
 O - o - o
 Paiva,
 O - o - o
 Paiva,
 O - o - o
 *Paiva, I've killed his money for him, his penis!*⁴²

⁴² Sung in Sena by Fernando Nicolos and the women of Pirira village, Luabo, August 5, 1975. For a detailed discussion of this particular song and for the Sena version, see Leroy Vail and Landeg White, "Plantation Protest: The History of a Mozambican Song," *Journal of Southern African Studies*, 5 (1978): 1-25. Also see Landeg White, "Power and the Praise Poem," *ibid.*, 9 (1982): 8-32.

Many different versions of this song exist in the villages that, until 1978, were subject to Sena Sugar's dominance. This particular version is fascinating in two ways. The concluding seven lines, which were sung by the people present many times after the soloist had completed the body of the song, represent the earliest version of the *Paiva*, dating from the 1890s. Thus, this simplest and oldest version has exactly the same form as the other work songs—the name, repeated, plus the epigram. And it was sung in this form in the cane fields from the 1890s to the 1950s. As an address to Sena Sugar, the song expresses an idea central to the Sena-Podzo peoples' perceptions of company and colonial overrule. The complaint—that the workers have made *Paiva's* money for him—is actually a complaint about appropriation without reciprocity. The phrase “I have killed for him” (*Ndampera*) refers metaphorically to the hunting of wild game, which, in Sena society, is governed by a set of rules specifying the just distribution of the kill. *Paiva*, the company, is demanding too much and providing too little in return: this is not what the *prazo* system is supposed to be about. Beyond this, the hierarchy in and of itself is not challenged.

Indeed, in the full version of the song just quoted, the address is to *Paiva* as protector. After the abolition of the *prazo* system in 1930 and the arrival of the Portuguese administrators and *chefes do posto*, the people of the area continued to look to the company as patron. The extremes to which such attitudes could be carried are illustrated by one version of the state takeover in which the whole problem of violence is attributed to an error of judgment made by the Africans themselves.

Now, when the government came, we were all called to the Company. They told us that the Company was no longer going to deduct the taxes from us, because now there was white government. “You can choose, if you want to stay with the Company, or if you want to move over to the government. But we will no longer be responsible for any complaints.” And then, we all voted to go to the government. And then the Company said to us, “You’ve now chosen to go to the government, but I can tell you in future you are going to cry for the Company.” But the government was really bad! Our hands are all swollen with this palmatoria. Then we went to the Company and said, “We’re being beaten up.” And then the Company said, “Didn’t we tell you? You chose the government, now you go to the government. Don’t come here to complain about it.”⁴³

Hence, this version of the *Paiva* song continues to invoke the relationship of client to patron that the people of the area had, with some success, imposed on the company when it was first founded. *Paiva* is addressed as “father,” as though company and workers were bound by kinship relations, and the whole thrust of the song is an appeal to *Paiva* to have pity. In other versions, *Paiva* is addressed repeatedly as “father” and “mother” and, in one instance, as *Mbuya*, the “Big One” or “Elder.” Given such perceptions of the relationship of workers and their employers, all the other Sena-Podzo songs understandably complain about the misconduct of specific individual members of the official hierarchy and about conditions of living and working but pose no larger challenge to the colonial system.

⁴³ Group Interview, Mopeia, September 20, 1975.

Just as, in the last resort, the Lomwe-Chuabo peoples wanted to be left alone in their rural homesteads, so in the last resort the Sena-Podzo peoples wanted the *prazo* system to continue, in a reformed, more equitable fashion with more adequate and responsible patrons but otherwise unchanged in its patterns of authority. This attitude, too, influenced their reactions to the anticolonial struggle and has conditioned their response to the challenge of independence.⁴⁴

THE MAJORITY OF THE SONGS examined thus far have been work songs, intended originally to accompany such activities as pushing trucks or pounding grain into flour, and all of them were sung by the people actually doing the work—whether on the sugar or sisal or tea plantations, on the cotton or rice concessions, or at home doing domestic chores. They record first-hand experience directly, and most of them are in the first person. The forty-six Chopi songs that Hugh Tracey recorded come from a rather different context. The composers of the *migodo* he discussed were professional musicians—Katini, Gomukomu, Sauli Ilova, and Sipingani Likwekwe. And their orchestras of massed xylophones, which contained as many as forty-eight musicians, could not have been brought together without some system of patronage.

Their orchestras are to be found in every large village. In the Zavala District alone each of the eight more important chiefs has his own *Ng'godo* of orchestra and dances. . . . Katini is the leader and composer at the kraal of the Paramount Chief Wani Zavala, and Gomukomu holds the same office at the kraal of Filippe we Madumane Banguza, an important chief whose country, called Mangene, lies along the north-western boundary of the Zavala District.⁴⁵

The orchestras are maintained by the chiefs, and the role of the orchestra leader is, in part, that of a court praise-singer. The songs themselves stress the specialist skills of the musician and the mystery of the composer's craft. "To play the /mtimbila you must dream about it," sings Gomukomu. Or, as Sipingani Likwekwe puts it, "You must dream to compose music." Katini's version is the most vivid statement of this theme:

Wani Zavala!
Hush, you people of Zavala,
Cease your chatter
At this court of chiefs!

Timbila music is so moving it brings tears,
This music of Katini's *timbila*
Singing and dancing.

Wani Zavala!
Hush, you people of Zavala,
Cease your chatter
At this court of chiefs!⁴⁶

⁴⁴ See Vail and White, *Capitalism and Colonialism in Mozambique*, chap. 9.

⁴⁵ Tracey, *Chopi Musicians*, 1–2.

⁴⁶ *Ibid.*, 27. Also see *ibid.*, 32, 75.

You, Chugela, you are proud of your position, yet
you are only a chief made by the white men.
Oh, the chieftainships of Nyaligolana and Chugela!
Oh, the chieftainships of Nyaligolana and Chugela!
It is a shame that should be hidden from Wani.⁵⁰

Because the *ngodo* is an established genre, its set form imposes to some extent demands on the content of the songs. It must, in other words, fulfill certain roles and respect audience expectations. One very obvious limitation of the genre is its male domination. Not only do the *migodo* have no women participants, but in only two of the forty-six songs is any attempt made to represent women's experience. One is the brief opening song of Sauli Ilova's *ngodo*:

¹⁷ *Ibid.*, 43.

¹⁸ D. J. Webster, "Kinship and Cooperation: Agnation, Alternative Structures, and the Individual in Chopi Society" (Ph.D. dissertation, Rhodes University, Grahamstown, South Africa, 1975), 16.

⁴⁹ Tracey, *Chopi Musicians*, 48.

⁵⁰ *Ibid.*, 68.

⁵¹ For songs that imitate Katini's *ngodo* of 1940, see, for example, Tracey, *Chopi Musicians*, 68, 75.

⁵² Tracey, *Chopi Musicians*, 53. The exact flavor of songs such as this one, as recorded by Tracey, must be

The other, again a brief dance song, is by Gomukomu:

I am most distressed,
I am most distressed as my man has gone off to work,
And he does not give me clothes to wear,
Not even black cloth.⁵³

These two themes, especially the second with its complaint about the migrant labor system, are very common in women's pounding songs throughout southern Africa. Apart from these two moments, however, nothing in the Chopi *migodo* matches the kinds of concerns expressed in the Sena-Podzo and Lomwe-Chuabo women's songs.

Each *ngodo* contains certain established features. The order of the different sections in Katini's *ngodo* of 1940, for example, is as follows:

1. MUSITSO WOKATA	First orchestral introduction
2. MUSITSO WEMBIDI	Second orchestral introduction
3. MUSITSO WORARU	Third orchestral introduction
4. NG'GENISO	Entry of the dancers
5. MDANO	Call of the dancers
6. JOOSINYA	The dance
7. JOOSINYA CIBUDO COMBIDI	The second dance
8. MZENNO	The song
9. MABANDLA	The councillors
10. CITOTO CIRIRI	The dancers' finale
11. MUSITSO KUGWITA	The orchestral finale ⁵⁴

Allowing for slight variations in, for instance, the number of orchestral introductions, this is the basic shape of six of the *migodo* Tracey recorded. The demands of this structure are frequently referred to in the songs themselves: "Mguyusa, my young brother, help me compose my music. / I have no *mzeno* for my *timbila*."⁵⁵ Certain stock phrases recur and certain issues, such as the question of facial tattoos, are repeatedly aired as the composers maintain running arguments.⁵⁶ Quite clearly, the composers found these structural patterns satisfying, providing some unity in diversity. But the need to fulfill the requirements of form does affect the content of the songs, and no complete *ngodo* could be constructed from the kind of songs sung by the Sena-Podzo or Lomwe-Chuabo peoples. Just as there are sections where the orchestra plays alone, so there are others when the dancing takes precedence over the words, and *vice versa*. The high points of each *ngodo* in terms of the lyric are the *mdano*, the call of the dancers, and the *mzeno*, the song, which is performed by the composer himself.

considered elusive, for it appears that Tracey bowdlerized the songs he presented in his published work, removing obscenities. Compare, for example, Webster, "Kinship and Cooperation," 383 n.

⁵³ Tracey, *Chopi Musicians*, 46. The black cloth refers to mourning garments.

⁵⁴ Tracey, *Chopi Musicians*, 10.

⁵⁵ *Ibid.*, 35.

⁵⁶ *Ibid.*, 32, 58.

Tracey placed considerable emphasis on the overall artistic unity of each *ngodo*, and he explained the origin of this unity by describing how the sections were composed. The composer, he said, began with the words (presumably, although he did not say so, the words of the *mzeno*). The music was suggested by the flow of the words, as the chiChiopi language is tonal and “the sounds of the words themselves almost suggest a melodic flow of tones.” This melodic kernel was then transferred to the xylophone, and contrapuntal melodies were devised. Those melodies then became the themes of both of the orchestral accompaniments to the songs and of the orchestral introductions. Finally, the dance leader listened to the music and began to devise dance routines for the other sections.⁵⁷

What Tracey did not note, and what is somewhat obscured by his procedure of examining the forty-six songs one by one, is that each *ngodo* has a particular subject. Katini’s *ngodo* of 1940, for example, opens (after the orchestral introductions) with the entry of the dancers to words we have already discussed:

It is time to pay taxes to the Portuguese,
The Portuguese who eat eggs
And chicken!
Change that English pound!

The next section, the call of the dancers, describes an incident when Katini and his wife were beaten up by Kapitini, the court messenger, for drinking cashew cider. The section that follows, the dance itself, is accompanied by these words:

O - oh, listen to the orders,
Listen to the orders of the Portuguese.

O - oh, listen to the orders,
Listen to the orders of the Portuguese.
Men! The Portuguese say, “Pay your pound.”

Men! The Portuguese say, “Pay your pound.”
This is wonderful, father!
Where shall I find the pound?

This is wonderful, father!
Where shall I find the pound?

O - oh, listen to the orders,
Listen to the orders of the Portuguese.⁵⁸

By this stage, the subject of Katini’s *ngodo* of 1940 is clearly established. Its theme is Portuguese colonial rule. The complaint thus far is about taxation, about official violence, and about the prodigality of the Portuguese settlers. The next section, the second dance, contains lines already noted about the use of the *palmatoria*—“Here is a mystery, the Portuguese beat us on the hands,/ Both us an our wives.” This is followed by the *mzeno*, which attacks the ignoring of legitimate procedures in the

⁵⁷ *Ibid.*, 4–7.

⁵⁸ *Ibid.*, 14.

selection of state-appointed chiefs and deals with some of the problems that arise when the wrong man is appointed. The next section carries this point further, complaining about Portuguese interference in matters of Chopi succession. Running through the whole *ngodo* are words like “orders,” “threaten,” “beat,” “sjambok,” “avenge,” and “make trouble” and lines like “I heard them trying to hush it up” and “Fambayane was brought bound before the judge.” The songs are full of peremptory instructions—“Change that English pound!” and “Pay your pound!” and “Don’t waste your time with *timbila*!” and “If you come across Chimuke, greet him with a ‘Good Day!’ / Greet him well because he likes to be in amongst the chiefs.”⁵⁹ Chimuke was the district administrator, the source of all of these orders and the local representative of the colonial system, which is being criticized from so many different angles. Only in the final song of the *ngodo* does the mood relax, with a brief amusing song about the rather dilatory courtship by her suitor of Katini’s sister-in-law, an attractive widow.

In Katini’s next *ngodo*, however, performed in early 1943, the subject is quite different. There is no mention of taxation or *sjambok* or the *palmatoria* or the state-appointed chiefs. Kapitini, the court messenger, reappears very briefly, but only to be ridiculed in the context of an amusing little scandal. The dominant theme of the *ngodo* of 1943 is not the nature of Portuguese colonial rule but pride in Chopi culture and in the triumph of Chopi music. The first song accompanied the entry of the dancers:

Hey, Dawoti!
Dawoti go and ask Madikise.
He will tell you about our grandfathers.
Chitombe, behold Madikise!⁶⁰

Dawoti was a court messenger attached to the office of Administrator Luiz de Vasconcelos (*madikise*, “law-giver”), and Chitombe is one of the great ancestors of the Chopi people. The song’s double irony is, first, that instead of being ruled by Chitombe the Chopi are now ruled by the Portuguese and, second, that for flunkies (like Dawoti) Chopi history and culture exist only through the antiquarian researches of the Portuguese administrator! This is a sarcastic beginning to the *ngodo*, but in the next section, the call of the dancers, Katini described how in July 1939 the Chopi musicians were summoned to entertain Portuguese President Carmona on his state visit to Mozambique:

Come, you people of Zavala,
Come, you people of Zavala, and go to Magule.

The Song of Madikise,
It is wanted by Ngundwana at Magule.⁶¹

Magule is the Chopi name for the place of the Gaza Paramount Ngunguyane’s defeat at Lake Coolela in 1895 by the Portuguese general, Mousinho de Albuquerque.

The Gaza, known throughout southern Africa as the “Shangaans,” from the

⁵⁹ *Ibid.*, 16.

⁶⁰ *Ibid.*, 20.

⁶¹ *Ibid.*, 21.

name of their first chief, Soshangane, were old enemies of the Chopi. After a series of bitter wars, the Gaza defeated the Chopi in 1891. Now, at Magule, the Chopi and the Gaza are again in competition, this time before the Portuguese president and in terms of culture.⁶² The encounter is described in the dance, which juxtaposes two quite distinct events. The first four and last two lines refer to Chopi experiences working as migrant laborers in the mines of the Witwatersrand and encountering prejudice on the part of the African “boss boys” because they speak Chopi; lines 5–8, however, refer to the performance of Chopi songs at the meeting with the Portuguese president:

Malanje says, “You swear at me if I speak chiChopi.”
So I will speak chiSotho.

Malanje says, “You swear at me if I speak chiChopi,”
So I will speak chiSotho.

Katini will come to Magule to play *timbila*.
The President is glad to see the waChopi.

The Shangaans are left to sing their “Ho’ho siyana”
Until very late for the President.

Malanje says, “You swear at me if I speak chiChopi,”
So I will speak chiSotho.⁶³

To the delight of Katini, as lines 6–8 make clear, the Chopi defeated the Gaza at the very place where the Gaza were defeated by the Portuguese.

But, meanwhile, what happened to the Portuguese? This emphasis on entertaining the Portuguese president may seem pathetic, but Katini was interested in the nature of the encounter. Although the Portuguese now governed the Chopi, it appears from the *ngodo* thus far that they investigated Chopi history and were fast succumbing to Chopi culture, especially Chopi music. These triumphs were secured not by open confrontation and resistance, such as the Gaza had once attempted so disastrously at Magule. Rather, they were achieved by stealthy compliance of the type illustrated in the song by the vignette of ethnic relations on the Rand: when the “boss boy” refused to listen when addressed in Chopi, Katini spoke to him in Sotho, a small concession where there were other, more substantial victories to be gained.⁶⁴

With these points established, Katini reached the climax of his argument in the song (*mzeno*). The song’s principal theme is the visit that Katini and his orchestra paid to Lisbon in the summer of 1940, when they performed in the celebrations of Portugal’s tricentennial anniversary of freedom from Spain: “We made new tunes for the *timbila* in the midst of the sea / As we passed foreign lands.”⁶⁵ Chopi ethnic

⁶² Webster, “Kinship and Cooperation,” 14.

⁶³ Tracey, *Chopi Musicians*, 23.

⁶⁴ The proliferation of clandestine schools after 1939 to teach people arithmetic and, among other subjects, Portuguese also attests to the Chopi’s sense of realism; Charles E. Fuller, “An Ethno-Study of Continuity and Change in Gwambe Culture” (Ph.D. dissertation, Northwestern University, 1955), 242.

⁶⁵ Tracey, *Chopi Musicians*, 25.

pride could have had no greater triumph than this victory for music over politics and warfare, celebrated in Portugal itself!⁶⁶ Although the events of the *ngodo* were three or four years out of date by the time it was performed in 1943, Katini did not, as Tracey suggested, comment haphazardly on topical events; instead, the composer pursued a complex argument about the nature of one type of response to the realities of Portuguese rule, drawing on recent Chopi history to make his point about the power of Chopi music to conquer the conquerors. Appropriately, therefore, this *ngodo*, in the section following the *mzeno*, contains the song “Wani Zavala/ Hush you people of Zavala” about the power of Katini’s music to “bring tears.” Appropriately, too, the *ngodo* closes with a demonstration of Katini’s moral authority within the community, as he used the dancers’ finale to deliver a stern warning about the attempted seduction of an under-aged girl.⁶⁷

All of these considerations make the songs of the Chopi *migodo* very different from the Sena-Podzo and Lomwe-Chuabo songs. Because the *migodo* were performed by professional musicians rather than by laborers and cultivators and because they were devised for lengthy public entertainment rather than simply for accompaniment for communal or work-gang activity, the Chopi songs are much more thematically and aesthetically ambitious. It cannot be firmly established, on Tracey’s evidence, that the songs of the *migodo* genuinely represent popular opinion, as some of the Sena-Podzo and Lomwe-Chuabo songs clearly do. Yet the very fact that they are the work of individual professional composers, working chiefly under patronage, provides greater scope for comment on the broad issues that are engaged. Scholars must, however, beware of romanticizing work songs as automatically the “voice of the people.” When the Sena-Podzo find in the *Paiva* song “a map” of their experience, the reason is not simply that the song was originally a work song, sung by everyone, but more significantly that life in the overrule of Sena Sugar was stiflingly uniform. No one in a place where *Paiva* was “the store, the factory, the railway line, the compounds, the cane-fields” could be said to be free from the intrusion of the company in their lives. In this sense, the *Paiva* song spoke for everyone!⁶⁸ Similarly, the Lomwe-Chuabo people, given the circumstances of their history, naturally focused much of their anger on the state-appointed chiefs and headmen who were the most local and most visible enemies of the rural homestead’s integrity.

Chopi experience was considerably more diverse. In the nineteenth century, the groups that came to be called Chopi had lived under separate chiefs and had distinct political systems, despite their relatively homogeneous culture. Certain of these groups had benefited from a close alliance with the Portuguese, enabling them to keep the Gaza and other hostile groups at bay.⁶⁹ With such support, they

⁶⁶ Interwoven with the theme of cultural victory is a second theme, the death of Katini’s close friend and associate, Magengwe, who was also a claimant to the chieftainship of Wani Zavala. He died in Lisbon, and his death in a foreign land at the moment of triumph coupled with Katini’s suspicions that his death was not wholly unwelcome at court brings human folly and weakness back to the center of the argument and turns the *mzeno* into a haunting dirge.

⁶⁷ Tracey, *Chopi Musicians*, 28–29.

⁶⁸ Vail and White, “Plantation Protest,” 1–25 *passim*.

⁶⁹ St. Vincent Erskine, “Journey to Umzila’s, South East Africa, in 1871–1872,” *Journal of the Royal Geographical Society*, 45 (1875): 53.

had been able to take advantage of trading opportunities in the Delagoa Bay area and at Inhambane and had developed an active exchange economy. Initially, they traded large amounts of ivory and some slaves and then, after the opening of the Suez Canal in 1869, they exported a wide range of products, including rubber, a variety of oil-seeds, beeswax, and copra.⁷⁰ In the 1880s, however, as the Portuguese stepped up their efforts to gain control of the interior, the balance of power altered. The Gaza, driven back from the banks of the Zambesi, attacked many of the Chopi chiefdoms; some declared themselves tributary to the Gaza, and others fought to maintain their independence.⁷¹ Finally, in 1895, the Portuguese defeated the Gaza at Lake Coolela and solidified their power over southern Mozambique. In the first two decades of the twentieth century, the Chopi moved into labor migrancy both to the Witwatersrand and to Lourenço Marques, where they constituted a substantial portion of the local labor force. Their strategy was interesting. Accepting the inevitability of taxation and forced labor, they determined to maintain the integrity of Chopi society. By timing their trips to Lourenço Marques and the Rand so as not to conflict with the local planting season, by coordinating their absence with kinsmen or friends who agreed to do necessary agricultural work back home on their behalf, and by investing some of the money they earned in tools that improved productivity or in bridewealth, thus increasing the exploitation of female and child labor, the Chopi were able to preserve much of their rural economy as well as their access to purchased goods.⁷²

After World War I, however, pressures increased on the Chopi. Mozambique and Angola were granted a measure of local autonomy and their own budgetary responsibilities, and attempts were made to bring about economic growth. In Mozambique, especially in the southern part of the country, this took the form of encouraging the establishment of capitalist agriculture on the plantation model. Portugal hoped that growth in the local economy would help free Mozambique from its dependence on labor migration to South Africa for income.⁷³ In Chopi country, this departure involved the establishment of the Sociedade de Zavala, which was granted monopoly rights over the sale of peasant-grown *mafurra* oil, and a railway was built from Inharrime to carry the oil. Prior to the granting of the concession, the Chopi had exported the *mafurra* oil themselves, at rates of seven to eight thousand tons annually, through Indian traders acting either independently or as agents of various foreign trading houses. The pattern, in short, repeated that in Quelimane district in the 1890s, when peasant trading in copra through Indian intermediaries was taken over by the big copra companies; the difference in Zavala was the specific governmental support of Portuguese settler interests. In the event,

⁷⁰ Frederick Elton, "Journal of an Exploration of the Limpopo River," *Journal of the Royal Geographical Society*, 42 (1873): 22–32.

⁷¹ Patrick Harries, "Slavery, Social Incorporation, and Surplus Extraction: The Nature of Free and Unfree Labour in South-East Africa," *Journal of African History*, 22 (1981): 320–22. Also see Augusto Cabral, *Raças, usos, e costumes dos indígenas do distrito de Inhambane* (Lourenço Marques, 1910), 35.

⁷² Webster, "Kinship and Cooperation," chap. 3; and Jeanne Penvenne to authors, December 26, 1981. Also see Daniel da Cruz, *Em Terras de Gaza* (Porto, 1910), 154–74, 225–73 *passim*; Emma H. Haviland, *Under the Southern Cross: or, A Woman's Life Work for Africa* (Cincinnati, 1928), 306–12; Dora Earthy, *Vulvage Women* (London, 1933), 20–60; and Fuller, "Continuity and Change in Gwambe Culture," 150–51, 180.

⁷³ Vail and White, *Capitalism and Colonialism in Mozambique*, 200–16.

the Chopi refused to sell their *mafurra* oil to the new company at fixed rates. Rejecting Portuguese rationalizations that they had been “scandalously robbed” by the Indian merchants, the Chopi used the new railway line to facilitate labor migration to South Africa. The new company collapsed amid accusations that the high commissioner, Brito Camacho, had been bribed by the Rand Chamber of Mines to prevent it from succeeding. South Africa’s interest in obstructing any large-scale investment in southern Mozambique lest it should affect labor supplies for the Rand was confirmed in May 1928. Portugal reached a new agreement with South Africa, permitting the recruitment of eighty thousand migrant workers annually.⁷⁴

The Chopi, then, escaped the overrule of a single company. Although they were, of course, subject to the same labor laws as the Africans of Quelimane district and they may be said to have belonged, in a sense, to the vast labor enclave of the Rand mines, the Chopi in fact had a comparative variety of outlets for employment. The variety lay not just in the choice of employers—the Incomati sugar estates, the rice and banana plantations of the Incomati valley, the Lourenço Marques railway, and of course the Rand—but more profoundly in the kinds of work available. We are speaking comparatively; we by no means are trying to suggest that the Chopi people had an easy time under the Portuguese. But there is a qualitative difference between working for decades as plantation laborers in a single, closed district and working in the city or encountering the full might of industrial capitalism on the Rand. These broader experiences demanded a more complex and extended poetical form. The world of the *migodo* is a relatively big place. That world includes the village and the city, the plantation and the mine, Mozambique and South Africa (and even, briefly, Lisbon). It takes account of two different versions of colonialism and of a variety of different kinds of Europeans—Portuguese, English, Dutch, Italians, and Germans. It records the contact with other African peoples, the Shangaans in Mozambique and the Sotho and Xhosa on the Rand. The Chopi could not, then, have had, as the Sena-Podzo and the Lomwe-Chuabo peoples did, any single enemy present at all these locations or among all these choices. The *migodo* contain no uniform explanation of why things went wrong. They do exhibit, however, a definite pattern of concern. Although the forty-six songs of the seven *migodo* Tracey collected are the work of at least five different composers, so that the first impression is one of variety and contrast, they all share one central and passionate preoccupation.

This point can be most effectively established by an examination of how these *migodo* from the early 1940s deal with the potentially radicalizing experience of labor migration to the Rand. Little in these songs suggests that working in the mines was an unpleasant experience. “I’ll go to the mines to work for money so that when I come back I can buy cashew-cider to drink,” states one song. “If we go to the city we see wonders as we pass Pretoria,” states another, from a *ngodo* actually recorded

⁷⁴ Brito Camacho, *Mozambique: Problemas Coloniais* (Lisbon, 1926), 19–33. Also see T. H. Henrikson, *Mozambique: A History* (London, 1978), 121; Sherilynn Young, “Fertility and Famine: Women’s Agricultural History in Southern Mozambique,” in R. Palmer and N. Parsons, eds., *The Roots of Rural Poverty in Central and Southern Africa* (Berkeley and Los Angeles, 1977), 66–81; and Vail and White, *Capitalism and Colonialism in Mozambique*, 205–11.

on the Rand. Only one line from one song—"If I go to the mines, where shall I find the courage to get into the cage?"—gives us any impression of what it might have been like to work underground.⁷⁵ Most of the songs that refer to the Rand deal with the setting of the mining compound, and the subject is usually ethnic rivalries:

Cast off your skins!
There is no relish left, you Shangaans,
it has been eaten by the Sotho.
It has been eaten by the Sotho and the Xhosa,
and we will not get it.⁷⁶

On the Rand, the enemies tended to be not the Portuguese or the Randlords but other African peoples. For a fuller analysis of what is damaging about the migrant labor system, we must turn to Gomukomu's *ngodo* of 1942–43.

Gomukomu introduced the subject in the call of the dancers, apparently as a joke:

It is Filippe's opinion
That the girls also should sign on and go to the mines.
It is Filippe's opinion.
What a good idea!⁷⁷

Filippe is Filippe Banguza, the paramount chief who was Gomukomu's patron. In the next song of the *ngodo*, however, a second call of the dancers, the joke suddenly turns out to be serious:

Ha! We quarrel again! The same old trouble.
The older girls must pay taxes.
Natanele speak for me to the white man to let me be.⁷⁸

The complaint is about the governor-general's Circular of October 1942, which, while tightening the labor laws, gave local administrators no guidance on how this was to be accomplished.⁷⁹ One consequence among many was an intensification of tax collection, including the taxation of women. Filippe Banguza's duties, as a state-recognized chief, included tax collection—and the joke about sending women to the mines (Natanele was a recruiter) suddenly becomes most bitter. This *ngodo* contains, as the song accompanying the dance itself, the complaint noted earlier of the woman whose husband has gone off to the Rand, leaving her with no clothes to wear, not even the "black cloth" of mourning. The *ngodo* thus comments, from more than one angle, on the position of women in the labor system. Gomukomu's argument is then brought to a head in the *mzeno*. He complains that he himself has been forced to give up playing the *timbila* to go and work as a laborer on the Incomati banana plantations. He complains that the administrator is troubling

⁷⁵ Tracey, *Chopi Musicians*, 73, 80, 68. It should be noted, however, that Tracey's ties to the mines need to be explored in future work.

⁷⁶ Tracey, *Chopi Musicians*, 30.

⁷⁷ *Ibid.*, 41.

⁷⁸ *Ibid.*, 43.

⁷⁹ T. de Bettancourt, *Relatório do Governador-General de Moçambique, 1940–42*, I (Lisbon, 1945): 45–47.

everybody “with his constant calling” to enlist. And he complains that the police are now beating people up indiscriminately—“even the hands of chiefs” and “even women”—as the new labor regulations are enforced. The Portuguese “turn their backs” when any question of the people’s welfare is raised:

We got on the train and arrived at Sewe,
And when we spoke about the matter of food,
About the matter of food, they turned their backs.
We overheard the Portuguese speaking about food,
Speaking about food while their backs were turned.⁸⁰

Significantly, the incident described here occurred during President Carmona’s visit in 1939. Gomukomu, in effect, retorted to Katini’s argument that the Portuguese have been seduced by the appeal of Chopi culture. Finally, Gomukomu explained that the whole problem of increased labor demands has to be understood in the context of “the German war”: “The bloody fools of white men are fighting. / Matijawo says they are like four-legged beasts.”⁸¹ The presentation of the whole issue with a great deal of humor not represented in our selective quotations only reinforces the power of this *ngodo* and the quality of its analysis.⁸²

This *ngodo* deals only with local effects of migrant labor. It does not mention what life was like on the Rand and makes no comment about whether working conditions there were very unpleasant or whether the pay was far too low. Instead, it focuses on the consequences for the local community of recruiting methods, of state-sponsored violence, and of the men’s absence for such long periods of time.⁸³ The threat to the Chopi community is what matters.⁸⁴ We noted earlier that the world of these *migodo* is comparatively large. By contrast with the songs from Quelimane district, which show no awareness of a bigger world outside the immediate relations of power in which the people were trapped, the *migodo* seem to encompass great diversity and individuality. The impression, though, is misleading. Underlying the surface variety of opinion and of mood, the *migodo* express a fundamental concern for the health of the Chopi nation. Although the word itself appears only once in the forty-six songs (and then as a linguistic designation), Chopi identity, the good of the Chopi people, is the single central preoccupation to which all else relates.

⁸⁰ Tracey, *Chopi Musicians*, 48.

⁸¹ *Ibid.*, 47.

⁸² For an example of the humor, see, for example, *ibid.*, 48:

There are women foolish enough to take grain from the bins
to sell for beer!
To take corn and waste it on drink.
Listen, oh . . . !
. . . The bottle is empty!!

⁸³ For a discussion of the long-range effects of prolonged labor migrancy from Chopi country, see Fuller, “Continuity and Change in Gwambe Culture,” *passim*; and D. J. Webster, “Migrant Labour, Social Formations, and the Proletarianization of the Chopi of Southern Mozambique,” *African Perspectives*, 1 (1978): 154–74.

⁸⁴ With the outbreak of World War II, the forced cotton regimen imposed in Quelimane district was also implemented in Chopi territory, and the initial results included brutal exploitation of the women and several severe famines. It is reasonable to assume that this topic was handled in later *Migodo*, but this research has not been done. Fuller, “Continuity and Change in Gwambe Culture,” 152–53.

The *migodo*, in short, express a feeling of ethnic nationalism that is completely absent from the Sena-Podzo or the Lomwe-Chuabo songs. It may, of course, be argued that the Chopi chiefs form a traditional elite, that the musicians are court praise-singers, and that the ethnic nationalism of the *migodo* is very much what we should expect of a group trying to perpetuate its position. But that nationalism has other and broader facets. In part, it reflects the historical conflict with the Gaza, who—as “Shangaans”—are attacked in a number of songs. In part, too, it appears as a complex of reactions to the experience of working in the mines. For decades, the Chopi found themselves competing with ethnic groups from all over southern Africa. On the Rand they were housed together in their own compounds, a system intended to exploit ethnic differences among workers as a labor control device. There the Chopi learned that their language and culture set them apart from others and that their musical prowess was seen as one of the most striking aspects of this culture.⁸⁵ Mining compounds contained areas for the competitive demonstration of such “ethnic” skills as drumming and dancing, and the Chopi, like all migrant workers, were encouraged to consider themselves as “tribesmen.” It was in this situation that they most clearly saw themselves as “Chopi,” regarding “the efforts of other peoples with a great deal of disdain.”⁸⁶

At the same time, the skill with which the Chopi had made migrant labor an integral part of their village life, so that as time passed a contract of recruitment came to be viewed as a “second initiation” through which a young man had to pass before he was accepted as fully mature, only reinforced this sense of Chopi identity.⁸⁷ Most Chopi, then, appear to have identified with the concerns of their legitimate chiefs, who became the very symbols of the nation’s history and integrity. In the *migodo* themselves, no distinctions are made between those who were inside and those outside the circle of power, as, for instance, are repeatedly made in early Zulu or Swazi or Sotho praise poetry.⁸⁸ There are no appeals to national unity and no attacks on individuals for failing to respect chiefly power, as again occur in later Zulu or Swazi or Sotho praise poetry. There is no straining of any sort after audience attention or audience agreement, and song after song proceeds on the assumption that the performance of the *migodo* is a community entertainment (“Come together with your wives” and the like).

Most important of all, however, is clearly the positive, anticolonial aspect of Chopi nationalism. The targets for the most fierce attack in the *migodo* are consistently those that represent Portuguese authority, such as the court messengers—“You Lekení, you are as black as coal, / Son of Nyamandane, you are a terror!”—and the labor recruiters—“Listen, they are off to their kraals as they are afraid they will be signed on”—and the state-appointed officials with their new demands for “respect”—

Just listen to the songs of Chigombe’s village,
To keep on saying “Good Day” is a nuisance.

⁸⁵ Fuller, “Continuity and Change in Gwambe Culture,” 242.

⁸⁶ Webster, “Kinship and Cooperation,” 9, 79.

⁸⁷ *Ibid.*, 20, 81–83.

⁸⁸ See White, “Power and the Praise Poem,” *passim*.

Makarite and Bubwane are in prison
 Because they did not say "Good Day." . . .
 They had to go off to Quissico to say "Good Day" there instead!⁸⁹

And, of course, the Portuguese themselves as colonial rulers were targets. The Chopi songs, in fact, attack all those targets that are differentiated in the Sena-Podzo and the Lomwe-Chuabo songs according to local circumstances, and they do so from a belief in a Chopi nation that is perfectly capable of managing its own affairs. Running through all the songs of the *migodo* are references to two contrasting systems of authority. On the one hand, representing the Chopi nation, are their genuine chiefs, with their councils of elders and their professional musicians. On the other are the usurpers of power—the administrators and *chefes do posto*, the court messengers, the police, the labor recruiters, the tax collectors, and the puppet chiefs and headmen. The fundamental problem is, "They have taken the country, we know not how, and shared it out."⁹⁰ In the total absence in the 1940s of any likely military or political solution to this problem, the *migodo* demonstrate confidence in Chopi institutions and revel in the special skills and vitality of Chopi culture. As was noted in the mid-1970s,

Above all else it is the Chopi sense of identity which sets them apart from the other peoples with which they have contact. It is expressed in a feeling of pride in their "Chopi-ness," an indefinable group identity. It is given focus in their language (chiChopi), patterns of marriage which link their various clans to each other, and most important in their music, which crystallizes their identity, and the lyrics which focus their in-group feelings against outsiders.⁹¹

As with the Sena-Podzo and the Lomwe-Chuabo, so with the Chopi, the perceptions and attitudes formed in response to colonial overrule have proved tenacious.

THIS BRINGS US BACK TO THE QUESTION of "resistance" and "collaboration." These terms are, indeed, clumsy for describing both the psychology and the practice of African reactions to capitalism and colonialism. Of the groups described here, all of which belong to the same country at the same time and under the same system of colonial overrule, only the Lomwe-Chuabo can be said to have "resisted" in the sense that they refused utterly to come to terms with the regime: their resistance was expressed in large-scale migrations to Nyasaland. They, however, lived close to the border, and it is difficult to see how the Chopi could have adopted a similar course. For both the Chopi and the Sena-Podzo peoples, neither "resistance" nor "collaboration" adequately describes their perceptions or their behavior. Nor do any other terms, like the various forms of "consciousness," which are commonly applied, do justice to the degree to which what they thought and did made perfectly good sense in terms of their previous experience and of the realities of political, economic, and military power. A group's consciousness, especially in situations of only the most embryonic class divisions, must be carefully situated in the common-

⁸⁹ Tracey, *Chopi Musicians*, 41, 59, 55.

⁹⁰ *Ibid.*, 74.

⁹¹ Webster, "Kinship and Cooperation," 8–9.

place specificities of the historical context. What is clear, above all, from these songs is something of the quality of African life in Mozambique under Portuguese rule. A song like the following, from Gomukomu's *ngodo* of 1940, is as valuable as Bernardo Honwana's stories in providing us with a picture both of intolerable political pressure and of life going on nevertheless:

Come together and make music for the new year!
We fear only that our names will be written by the white men.

Ngukyusa, my young brother, help me compose my music,
I have no *mzeno* for my *timbila*.

You said that you would care for me, my Gomukomu,
You said that you would care for me, my Gomukomu,
But now in my house I am left weaving alone.

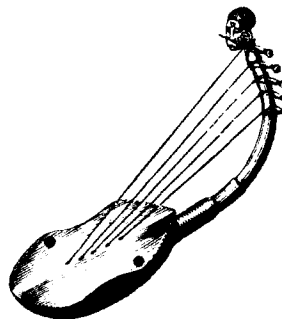
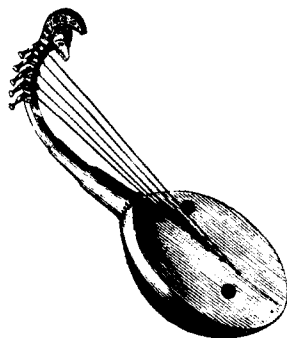
Manyina Mtumbu, you think you are beautiful because you are fair!
Manyina Mtumbu, you think you are beautiful because you are fair!
But you surely are sweet as the bees!

Lekeni the messenger has come to call you.
Filippe, our child, they will be the death of you with their calling.

You, Lekeni, are sent on important affairs.
You, Lekeni, are sent on important affairs.
Yet you dally on the road, joking with the girls.
You people of Zandamela are called to the Court.
Ngongondo, you fear the Court on account of your drinking.

Come together and make music for the new year!⁹²

In this song, the complaints about labor recruitment, about the behavior of court messengers like Lekeni, and about the system's lack of respect for Filippe Banguza, the paramount chief, provide the framework for the other comments—from Gomukomu's wife that she has been deserted while her husband composes his music, or about Manyina Mtumbu, whose light-colored skin is making her vain, or about Ngongondo, the drunken chief of a neighboring area. We get a sense of the texture of life in a Mozambican village under the *Estado Novo*, which is what social history should finally be all about.



⁹² Tracey, *Chopi Musicians*, 41.

No Chosen People: The Myth of the Calvinist Origins of Afrikaner Nationalism and Racial Ideology

ANDRÉ DU TOIT

THE ORIGINS OF AFRIKANER NATIONALISM and its associated racial ideologies and, thus, of the apartheid order imposed by the National party governments on South African society in recent times have often been ascribed to an obdurate strain of "Calvinism." According to this view, the Afrikaner founding fathers brought with them to the Cape the basic tenets of seventeenth-century Calvinist thought, in the isolated frontier conditions of *trekboer* society this mode of thought became fixated and survived for generations as a kind of "primitive Calvinism," and, Rip van Winkle-like, it then emerged to renewed historical prominence in the early nineteenth century and provided much of the rationale for that central event of Afrikaner history, the Great Trek. Thus, the Voortrekkers and the Republican Afrikaners conceived of themselves as a chosen and covenanted people, like the Israelites of the Old Testament, and early Afrikaners presumed a divine mandate to smite heathen peoples and reduce them to their pre-ordained position as perpetual hewers of wood and drawers of water. This cluster of constructs, which has been used to explain and justify racial inequality and repression in latter-day Afrikaner-dominated societies, constitutes a historical myth that I call the "Calvinist paradigm" of Afrikaner history.¹

Despite its pervasive presence in the literature, the content of the Calvinist

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¹ The present essay will be limited to a critical survey of the relevant secondary evidence for the existence of a "primitive Calvinism" and of the ideology of a Chosen People among early Afrikaners during the crucial period from the mid-eighteenth to the mid-nineteenth century. For a complementary survey of the relevant evidence in the primary sources for the history of early Afrikaner thinking prior to 1880, see my "Captivity to the Nationalist Paradigm: Professor F. A. van Jaarsveld and the Historical Evidence for the Afrikaner's Ideas on His Calling and Mission," *South African Historical Journal* (forthcoming); and, more generally, see André du Toit and Hermann Giliomee, *Afrikaner Political Thought: Documents and Analyses*, volume 1: 1780–1850 (Berkeley and Los Angeles, 1983). For the crucial transitional period at the end of the nineteenth century when neo-Calvinist notions were introduced from the Netherlands, notions that appear to provide a measure of authentication after the event and lead modern Afrikaner ideologists and historians to appropriate the myth for their own purposes, see my "Puritans in Africa? Afrikaner 'Calvinism' and the Reception of Kuyperian Neo-Calvinism" (forthcoming).

paradigm of Afrikaner history has seldom been fully and explicitly articulated. Indeed, most of the early Afrikaans versions of the myth barely specify the intellectual content of the purported "Calvinist" tradition. Its provenance is simply assumed—and little documented.² Seminal liberal historians of the 1930s, such as C. W. de Kiewiet and I. D. MacCrone, used crucial components of the Calvinist paradigm in a specific variant of the more comprehensive "frontier" interpretation of South African history.³ Only in the 1950s did scholars begin, on the one hand, to document specific "Calvinist" theses in historical monographs and, on the other, systematically to elaborate the Calvinist paradigm itself as the centerpiece of critical accounts of the historical origins of modern Afrikanerdom and apartheid and as a *leitmotiv* of the Afrikaner's own understanding of South African history.⁴ With the growth, particularly since the 1960s, of an international literature on race and politics in South Africa, the Calvinist paradigm has found its way, in one form or another, into a wide range of social science literature and has been incorporated as a basic analytic premise of T. Dunbar Moodie's study of Afrikaner civil religion.⁵ Recently, this paradigm has reached a much wider public still by such popularized works as W. A. de Klerk's *Puritans in Africa* and by James Michener's *The Covenant*.⁶

² See, for example, Dr. O'Kulis [pseudonym for the Reverend Willem Postma], *Doppers* (Bloemfontein, 1918), chap. 1; C. G. Nepgen, *Die sosiale Gewete van die Afrikaanssprekendes* (Stellenbosch, 1938), chaps. 6, 7, 9; G. B. A. Gerdener, *Ons Kerk in die Transgariep: Geskiedenis van die Ned. Geref. Kerke in Natal, Vrystaat en Transvaal* (Cape Town, 1934); L. J. du Plessis, *Die Maatskaplik-Staatkundige Ontwikkeling van die Kalvinisme in Suid-Afrika* (Potchefstroom [1928?]); J. V. Coetzee, "Die Geskiedenis van die Calvinisme in Suid-Afrika," in H. G. Stoker *et al.*, eds., *Koers in die Krisis*, 1 (Stellenbosch, 1935): 55–65; P. J. Meyer, *Die Afrikaner* (Bloemfontein, 1940), 33–34, 64–66, chap. 6; and H. G. Stoker, *Die Stryd Om die Ordes* (Pretoria, 1942), 1–3, 250–51, 274–76.

³ MacCrone, *Race Attitudes in South Africa* (Johannesburg, 1937); de Kiewiet, *A History of South Africa: Social and Economic* (Oxford, 1941); and Eric Walker, "The Formation of the New States," in *The Cambridge History of the British Empire*, 8 (1936): 219. Also see Eric Walker, *The Great Trek* (London, 1934), 55–59. For a critical perspective, see Martin Legassick, "The Frontier Tradition in South African Historiography," in Shula Marks and Anthony Amore, eds., *Economy and Society in Pre-Industrial South Africa* (London, 1980), 44–79.

⁴ For the documentation of the paradigm, see, for example, W. Kistner, *The Anti-Slavery Agitation against the Transvaal Republic, 1852–1868*. Archives Year Book for South African History [hereafter, AYB], (1952), no. 2: chap. 3; M. C. E. van Schoor, *Die nasionale en politieke Bewuswording van die Afrikaner in Migrasie en sy Onthulling in Transgariep tot 1854*, AYB, (1963), no. 2: chap. 3; M. Boucher, *The Frontier and Religion*, AYB, (1968), no. 2: chaps. 1–3; J. P. F. Moolman, "Die Boer se Siening van en houding teenoor die Bantoe in Transvaal tot 1860" (M.A. thesis, University of Pretoria, 1975), chaps. 1–2; B. Spoelstra, *Die Doppers in Suid-Afrika, 1760–1899* (Johannesburg, 1963); Gerhard Beckers, *Religiöse Faktoren in der Entwicklung der südafrikanischen Rassenfrage: Ein Beitrag zur Rolle des Calvinismus in Kolonialen Situationen* (Munich, 1969); and G. D. Scholtz, *Die Ontwikkeling van die politieke Denke van die Afrikaner*, 1 (Pretoria, 1967): 18–25, 119–30, 185–96. For its elaboration as an explanatory tool, see Sheila Patterson, *The Last Trek: A Study of the Boer People and the Afrikaner Nation* (London, 1957); and, for its role in the Afrikaners' historical self-understanding, see F. A. van Jaarsveld, *The Afrikaner's Interpretation of South African History* (Cape Town, 1964). Also see J. Alton Temple, "God and the Covenant in the South African Wilderness," *Church History*, 37 (1968): 281–97.

⁵ Moodie, *The Rise of Afrikanerdom: Power, Apartheid, and the Afrikaner Civil Religion* (Berkeley and Los Angeles, 1975). For the paradigm's incorporation in social science literature, see, for example, P. van den Berghe, *South Africa: A Study in Conflict* (Middletown, 1965), 14–15; A. G. J. Crijns, *Race Relations and Race Attitudes in South Africa* (Nijmegen, 1959), 41–42; Edward A. Tirvakian, "Apartheid and Religion," *Theology Today*, 14 (1957): 358–400; Jan J. Loubser, "Calvinism, Equality, and Inclusion: The Case of Afrikaner Calvinism," in S. N. Eisenstadt, ed., *The Protestant Ethic and Modernization: A Comparative View* (New York, 1968), 367–83; Randall G. Stokes, "Afrikaner Calvinism and Economic Action: The Weberian Thesis in South Africa," *American Journal of Sociology*, 81 (1975): 62–81; and Susan R. Ritner, "Salvation through Separation: The Role of the Dutch Reformed Church in South Africa in the Formulation of Afrikaner Race Ideology" (Ph.D. dissertation, Columbia University, 1977), chaps. 1–3. Also see Heribert Adam, "Perspectives in the Literature: A Critical Evaluation," in Heribert Adam and Hermann Giliomee, *Ethnic Power Mobilized* (New Haven, 1979), 17–25.

⁶ De Klerk, *The Puritans in Africa* (London, 1975); and Michener, *The Covenant* (New York, 1980). Also see Barbara Villet, *Blood River: The Passionate Saga of South Africa's Afrikaners and of Life in Their Embattled Land* (New York, 1982).

Several scholars—Martin Legassick, Irving Hexham, Richard Elphick, and Hermann Giliomee—have expressed some doubts regarding the historical and sociological premises of the Calvinist paradigm of Afrikaner history. But their reservations have had little impact thus far; such an experienced and distinguished historian as George M. Fredrickson recently subscribed to a slightly qualified version of the myth.⁷

Like other myths, the Calvinist paradigm is probably most effective when left largely implicit. But a critical investigation requires that the main historical theses be articulated more specifically. Inevitably, this will be a composite and controversial construction, for there is no single authoritative version of the myth. But the following more or less coherent composite account, gleaned from the most influential and fullest discussions in the recent literature, provides the historical framework of the myth even where it is not stated in so many words.

THE POINT OF DEPARTURE FOR THE CALVINIST PARADIGM of Afrikaner history is the claim that, in view of the time and of their places of origin, the founding fathers of the Afrikaner group were Calvinists. The claim should not be overstated. No one maintains that those who settled at the Cape in the late seventeenth and early eighteenth centuries were, like the Puritans in New England, engaged in a communal enterprise of deliberately founding new societies in accordance with their religious beliefs. That obviously goes too much against what is commonly known about the Dutch East India Company's objectives in starting a refreshment post at the Cape. But what is of considerable importance to the Calvinist paradigm is that the small number of settlers who arrived in the first few decades of settlement were not just, in some general sense, Christians or Protestants but were specifically Calvinists. MacCrone singled out the French Huguenots of 1688 for mention in this connection: "the character of [their] religion . . . was strictly Calvinistic . . . ; they had actually suffered persecution in their own persons for its sake." But more often the claim is less specific, encompassing the Dutch and other North-European settlers as well.⁸ Nor did the settlers, taken as individuals, need to be particularly pious or knowledgeable in matters of theological doctrine. The basic proposition merely asserts that collectively these settlers brought with them from their countries of origin certain general cultural traits and religious practices specific to Calvinism and that, significantly, they emigrated from the Netherlands (or France and Germany) at a very early juncture—that is, *before* the major Calvinist tradition had relapsed into scholasticism or had begun to adjust to the changing intellectual and social world of the Enlightenment and the Industrial Revolution.

⁷ Fredrickson, *White Supremacy: A Comparative Study in American and South African History* (New York, 1981), 170–79. For recent critiques of the paradigm's premises, see Legassick, "The Frontier Tradition in South African Historiography," 52–62; Hexham, "Dutch Calvinism and the Development of Afrikaner Nationalism," *African Affairs*, 79 (1980): 195–208, and *The Irony of Apartheid* (New York, 1981); and Giliomee and Elphick, "The Structure of European Domination at the Cape, 1652–1820," in Elphick and Giliomee, eds., *The Shaping of South African Society, 1652–1820* (Cape Town, 1979), 362–64. Also see T. Dunbar Moodie, "Calvinism and Afrikaner Nationalism—A Comment," *African Affairs*, 80 (1981): 403–04.

⁸ MacCrone, *Race Attitudes in South Africa*, 87; Tirvakian, "Apartheid and Religion," 386–87; Loubser, "Calvinism, Equality, and Inclusion," 371; and Nèpgen, *Die sosiale Gewete van die Afrikaanssprekendes*, 67–68.

The settlers came from societies, it is argued, where the original doctrines of Calvin himself and of the Synod of Dordt were still paramount. Accordingly, even if the cultural baggage of the Afrikaner founding fathers was rather meager, it essentially consisted of this "seventeenth-century" Calvinism.⁹

Sheila Patterson neatly summarized the next stages in the Calvinist paradigm of Afrikaner history: "It was the Old Testament and the doctrines of Calvin that moulded the Boer into the Afrikaner of today. . . . The doctrines which the Boers took with them on their long trek through the veld and the centuries were those of sixteenth century [*sic*] Calvinism, reduced to their simplest form in the memory of simple men with only the Bible to guide them."¹⁰ The major proposition advanced here, that the Calvinism originally introduced by the first settlers in due course became a constituent feature of later Afrikaner society and thinking, is a cornerstone of the Calvinist paradigm of Afrikaner history, constantly asserted or implied throughout the literature.¹¹ In more specific terms, the original Calvinism of the early settlers was initially adapted to colonial Africa and then became fixed as a kind of "primitive Calvinism," which was transmitted essentially unchanged to successive generations and much later emerged as the fundamental component of Afrikaner racial thinking and practice. This argument postulates a remarkable cultural time-lag across the centuries, and obviously requires special explanations.¹² The peculiar retention and transmission of this "primitive Calvinism," evident in Patterson's summary, is generally explained in two ways: negatively, this Rip-van-Winkle effect is the result of the Trekboers' almost total cultural isolation for many generations; positively, it derives from the central place of the Bible, particularly the Old Testament, in the religious practice and social life of the patriarchal family under frontier conditions.

"In the long quietude of the eighteenth century," wrote de Kiewiet, "the Boer race was formed." The emphasis here is "negative," on the prolonged cultural isolation and, indeed, insulation. In the course of the eighteenth century, while the rest of the Western world was profoundly affected by the secular and universalistic ethos of the Enlightenment, swept by the rising liberal and democratic tide of the "Atlantic Revolution," and finally transformed by the vast social changes attendant

⁹ The exact dating of this original Calvinism in the literature is, to say the least, somewhat rough and ready. Sheila Patterson spoke of sixteenth-century Calvinism, whereas Randall Stokes referred to the European Calvinism of the eighteenth century as the norm; Patterson, *The Last Trek*, 177; and Stokes, "Afrikaner Calvinism and Economic Action," 62.

¹⁰ Patterson, *The Last Trek*, 177.

¹¹ Loubser, for example, asserted, "The responses of the Calvinist religious system determined to a significant degree the characteristic features of Afrikaner society"; "Calvinism, Equality, and Inclusion," 379. And MacCrone claimed, "The frontier farmer still retained the peculiar tradition of his seventeenth century European background in its original form. It was that tradition that played a fundamental part in determining his race attitudes"; *Race Attitudes in South Africa*, 126–27. Also see van Jaarsveld, *The Afrikaner's Interpretation of South African History*, 5–6; Crijns, *Race Relations and Race Attitudes in South Africa*, 40; and Templin, "God and the Covenant in the South African Wilderness," 282–84.

¹² De Kiewiet was particularly taken with this cultural Rip-van-Winkle effect: "Deep into the nineteenth century they [the Trekboers] took the non-literary and non-industrial habits of the eighteenth century. Thus were fixed those attitudes and habits of mind which later returned from exile profoundly to influence all South Africa." *History of South Africa*, 56; also see *ibid.*, 58, 71. And see Patterson, *The Last Trek*, 178. An influential source for this notion appears to have been J. F. van Oordt's *Paul Kruger en de Opkomst der Zuid-Afrikaansche Republiek* (Cape Town, 1898); see pages 950–51, below.

on the Industrial Revolution, Trekboer society took shape in the vast open spaces of the Cape interior quite removed from all of these influences. From this lack of contact with modern developments follows an essentially negative conclusion: "Afrikaner culture was *not* significantly influenced by the rationalism and naturalism of the Enlightenment nor by modern liberalism."¹³

The social agencies that ensured the retention and transmission to succeeding generations of the original Calvinist core of beliefs and values, if perhaps in a somewhat reduced and impoverished form, were the Dutch Reformed Church and the patriarchal family unit. The customary practice of Bible readings, particularly of the Old Testament, and saying prayers at home held the central place in the religious and cultural life of Trekboer society.¹⁴ Beyond the family, the quarterly gatherings for *Nachtmaal* in some regional meeting place constituted—except perhaps for commando service—the remaining social life and exchange in Trekboer society. Baptism, confirmation, and marriage were the important rites of passage, marking the individual's incorporation in the larger Afrikaner community. Since confirmation requirements included some measure of literacy and confirmation was a prerequisite to marriage, literacy and religion were closely intertwined and together constituted two of the main features that differentiated these early Afrikaners from the indigenous groups and communities with which they interacted. To early Afrikaners, their Christian religion thus came to have great social and symbolic significance. "For the frontier farmer," MacCrone concluded, "religion was, first and foremost, a social fact—and a jealously guarded group-privilege." Afrikaner writers have made much the same—positive—point, stressing the constructive and integrative function of religion in preventing cultural and moral degeneration and general lawlessness: "It . . . bound the community together, fostered its unity, secured it against miscegenation and degeneration and inspired it with courage to combat the British and barbarian alike. Above all, it was a civilizing influence that bestowed positive values, law and order on the community."¹⁵

Within this larger religious context, the Old Testament was central to the practices and thinking of early Afrikaners. Writers have emphasized that the favorite passages for Bible readings were taken from the Old rather than the New Testament, that these passages tended to be given a literal reading and a fundamentalist interpretation, and that they were often understood as Israelite parallels to the Afrikaners' own nomadic and pastoral mode of life. The Old Testament thus informed and inspired their self-conception: "To the Boers the Old Testament was like a mirror of their own lives. In it they found the deserts and the

¹³ De Kiewiet, *History of South Africa*, 17; and Loubser, "Calvinism, Equality, and Inclusion," 372 (italics added). Also see Patterson, *The Last Trek*, 178; and Spoelstra, *Die Doppers in Suid-Afrika, 1760–1899*, 3, 25.

¹⁴ Scholtz, *Die Ontwikkeling van die politieke Denke van die Afrikaner*, 185–87; Nepgen, *Die sosiale Gewete van die Afrikaanssprekendes*, 58–64, 93–95; Tiryakian, "Apartheid and Religion," 391–92; and Rimer, *Salvation through Separation*, 20–25.

¹⁵ MacCrone, *Race Attitudes in South Africa*, 127; and van Jaarsveld, *The Afrikaner's Interpretation of South African History*, 10. Also see Crijns, *Race Relations and Race Attitudes in South Africa*, 40–42; Tiryakian, "Apartheid and Religion," 390–91; Spoelstra, *Die Doppers in Suid-Afrika, 1760–1899*, 23; and Stokes, "Afrikaner Calvinism and Economic Action," 68. For citations to early Afrikaner authors (van Oordt, Tobie Muller, and others), see Nepgen, *Die sosiale Gewete van die Afrikaanssprekendes*, 63–64.

fountains, the droughts and the plagues, the captivity and the exodus.”¹⁶ Early Afrikaners then only had to take a small—but crucial!—step to identify their own history with that of Israel in the Old Testament. This was to view themselves, like Israel, as a Chosen People with a divine mission. Indeed, many writers held that this notion provided much of the motivation for, and the self-understanding of, such a crucial historical event as the Great Trek. As F. A. van Jaarsveld phrased it, “The British administration had stood in the shoes of Pharaoh and oppressed them in Egypt—a country that they had to forsake to seek freedom. And so the exodus to the Promised Land was undertaken. The Voortrekkers and their descendants in their new home (Israel) felt that they were waging a struggle for survival against ‘Pharaoh’ and ‘the black Canaanites.’”¹⁷

This purported early Afrikaner ideology of a Chosen People is central to the Calvinist paradigm of Afrikaner history. The nature of the link between the Old Testament themes and the Trekker ideology depends on the meaning assigned to “Calvinism” in this context. Calvinism comprises, of course, a complex body of theological doctrine with a long and diverse intellectual history, but the critical tenet in terms of this literature is the Calvinist notion of *predestination* with its concomitant concept of *the elect*.¹⁸ Belief in individual predestination must have been prevalent among, and important to, early Afrikaners as “primitive Calvinists.” Evidently, they extended the notion of individual election and salvation to that of collective election. Thus, M. Boucher, for example, argued that on the frontier “the idea of a personal call by God, which was a significant part of the isolated farmer’s religious convictions, was becoming a national call.”¹⁹ In general, the Calvinist dichotomy between the chosen and the damned, those elected and those not, according to their predestined role, supposedly provided these early Afrikaners with an appropriate conceptual scheme for the interracial circumstances of the frontier. Often forms of this interpretation simply elide the intervening notion of individual election and make a direct connection between these early Afrikaners’ literal reading of the Old Testament and their identification with the Chosen People.²⁰ Whether or not individual election is bypassed, however, the early Afrikaner ideology of a Chosen People is closely linked with the historical event of the Great

¹⁶ Patterson, *The Last Trek*, 177. Also see de Kiewiet, *History of South Africa*, 17, 22–23; van Jaarsveld, *The Afrikaner’s Interpretation of South African History*, 9; van Schoor, *Bewuswording van die Afrikaner*, 14–15, 52; Nepgen, *Die sosiale Gewete van die Afrikaanssprekendes*, 64; Walker, *The Great Trek*, 55–56; and Moolman, “Die Boer se Siening,” 25–28. For the influence, in this connection, of some of passages in the earliest writings of Theal, see pages 948–50, below.

¹⁷ Van Jaarsveld, *The Afrikaner’s Interpretation of South African History*, 9–10.

¹⁸ See, for example, Patterson, *The Last Trek*, 177: “Chief among these doctrines [of sixteenth-century Calvinism] were that of the elect and predestination.” Also see MacCrone, *Race Attitudes in South Africa*, 87, 129; Kistner, *Anti-Slavery Agitation against the Transvaal Republic*, 209; Beckers, *Religiöse Faktoren in der Entwicklung der südafrikanischen Rassenfrage*, 102–04; Tiryakian, “Apartheid and Religion,” 391; and Stokes, “Afrikaner Calvinism and Economic Action,” 72.

¹⁹ Boucher, *The Frontier and Religion*, 49. Also see van Schoor, *Die Nasionale en politieke Bewuswording van die Afrikaner*, 14–15, 39–40, 52–53. Moodie suggested that Calvin’s own doctrine of “intermediate election” provided a basis for this transition, since intermediate election is applicable not just to individuals but to an ethnic group as well: *The Rise of Afrikanerdom*, 24–26.

²⁰ See, for example, Crijns, *Race Relations and Race Attitudes in South Africa*, 41; Patterson, *The Last Trek*, 177; van Jaarsveld, *The Afrikaner’s Interpretation of South African History*, 10; Moodie, *The Rise of Afrikanerdom*, 12; de Kiewiet, *History of South Africa*, 23; and Stokes, “Afrikaner Calvinism and Economic Action,” 73.

Trek. Sometimes it is claimed that much of the motivation and self-understanding of the Trekkers themselves were derived from notions such as these: "Assured of the guidance of God they left their homesteads and trekked into the unknown wilderness. . . . In the name of God they would fight the overwhelming armies of the Matabele and the Zulus and give the glory to Him when a victory was gained."²¹ Sometimes more sophisticated versions of the paradigm rather emphasize the significance of the Trek in later Afrikaner interpretations of their "sacred history": The Great Trek "forms the national epic—formal proof of God's election of the Afrikaner people and His special destiny for them."²² In either case, the central historical proposition—that the early Afrikaners did conceive of themselves as a Chosen People and that this self-conception must be understood in the context of a Calvinist theological tradition of predestination and the elect dating from the sixteenth and seventeenth centuries—remains the same.

The social and political import of the religious notions of the elect and a Chosen People is commonly spelled out in terms of a national mission or destiny. Crucial to the Calvinist paradigm of Afrikaner history is the early (and later) Afrikaner belief in such a special destiny and a divine national mission: "The [Afrikaner] civil theology is . . . rooted in the belief that God has chosen the Afrikaner people for a special destiny."²³ When the actual content or nature of this Afrikaner mission is specified, however, accounts diverge and even contradict one another. Even though Afrikaners initiated missionary activities only at a late stage and early Afrikaner attitudes to missionaries and their work were, to say the least, somewhat mixed, some writers have associated the Afrikaners' national mission with the spread of Christian gospel among the heathens: "They were called and led by Jehovah, their King, Ruler and Judge, to glorify him by establishing his kingdom on the dark continent among the heathen." Because of Afrikaner ambivalence to missionary activity, Fredrickson was careful to qualify the idea of a Christian mission to exclude the actual spreading of the gospel: "Their mission was not so much to spread Christianity among the heathens as to preserve themselves as a Christian community amid a horde of Savages."²⁴ Other writers have formulated a more secular version of the Trekkers' notion of a national mission, limiting it to the spread of civilization and order: "The [Boers] took upon themselves the task of putting an end to the constant intertribal warfare which had depopulated the vast

²¹ Kistner, *Anti-Slavery Agitation against the Transvaal Republic*, 209. Also see Nepgen, *Die sosiale Gewete van die Afrikaanssprekendes*, 64; Dr. O'Kulis, *Doppers*, 21–30; van Oordt, *Paul Kruger*, 27; and Templin, "God and the Covenant in the South African Wilderness," 284–86. For a succinct, popularized version of the Calvinist paradigm in this regard, see Gideon S. Were, *A History of South Africa* (New York, 1974), 52–53: The Boers "believed that they were the 'chosen race,' God's own people. As such they had a clear duty to God and mankind to preserve their traditions and culture. They had an obligation to preserve their race by avoiding mixing with other races, above all with non-whites. . . . In joining the Trek, they believed they were fulfilling a divine mission: to keep God's chosen race free from contamination through intermixing with the 'inferior' races, the 'Gentiles.' . . . Had the Almighty God not made them His chosen race? Had He not given them Africans and other non-whites as their eternal servants?" Also see *ibid.*, 45–47, 54–55.

²² Moodie, *The Rise of Afrikanerdom*, 3.

²³ *Ibid.*, 12. Also see, for example, de Kiewiet, *History of South Africa*, 23; and Patterson, *The Last Trek*, 178.

²⁴ Loubser, "Calvinism, Equality, and Inclusion," 371; and Fredrickson, *White Supremacy*, 173. But compare S. F. N. Gie, "The Cape Colony under Company Rule, 1708–1795," in the *Cambridge History of the British Empire*, 165.

areas that were now open for European penetration.”²⁵ But most characteristically and importantly, the idea of an Afrikaner national mission as a Chosen People is linked with their conquest of the indigenous peoples and the unequal racial orders established in the Republics.

A central thesis of the Calvinist paradigm of Afrikaner history is that the ideology of a Chosen People functioned to legitimate racial inequality and oppression. The early Afrikaners’ rejection of any form of *gelijkstelling* (“leveling”) of blacks and whites must be understood in terms of the underlying Calvinist dichotomy between the elect and the nonelect. As Jan J. Loubser phrased it,

much of the motivation for the founding of the Afrikaner Republics in the nineteenth century—as well as the present one—was aimed at gaining control of the polity to institute the divine order in society as a whole. The two-class system of the elect and the reprobate was applied in this context. . . . It is imperative by divine command to gain and maintain control over them [the nonwhites] by force if necessary to keep them in lower-class position in the interest of creating the Utopia.²⁶

Few other writers have followed Loubser’s example and introduced notions of a utopian society, at least in the nineteenth-century context, but all who subscribed to the Calvinist paradigm have claimed that by extension the Calvinist doctrine of election came to serve as a crucial legitimation for conquest and racial inequality. In Fredrickson’s considered view it can be maintained—and, indeed, on the basis of the Calvinist paradigm it must be held—that early Afrikaners subscribed “to the notion that the Boers were a chosen people, analogous to the ancient Israelites, who had a special and exclusive relationship with God *and a mandate to smite the heathen*.”²⁷

In the context of this central claim for the Calvinist origin of racial inequality, writers usually also adduce other components of traditional Afrikaner popular beliefs, such as the curse on the sons of Ham. From an early stage Trekboers on the frontier invoked the curse of Ham or beliefs that blacks were “creatures” not fully human to support the views that blacks are destined to be perpetual hewers of wood and drawers of water and that they therefore are properly treated by open coercion. Such views are compatible with the notion of the Afrikaners themselves as a Chosen People and together provide justifications for injunctions against racial equality and miscegenation. “It was as a chosen race that the Israelites had received a divine injunction not to intermarry with the Canaanites. The non-whites of South Africa were identified therefore not only with the children of Ham but also with the Canaanites of the Promised Land.”²⁸

²⁵ Kistner, *Anti-Slavery Agitation against the Transvaal Republic*, 210.

²⁶ Loubser, “Calvinism, Equality, and Inclusion,” 374. But compare MacCrone, *Race Attitudes in South Africa*, 87, 127, 129. Also see A. J. H. van der Walt, “Exodus van die Boerevolk,” *Koers*, 4 (1938): 6: “The idea of being a chosen people, of being set apart, of the task to keep themselves free from mixing with the heathen peoples took very deep root, and actually provided the deepest foundations for the powerful racial consciousness of the Afrikaner people and their urge to survive as a *separate people*.”

²⁷ Fredrickson, *White Supremacy*, 171 (italics added).

²⁸ Van Jaarsveld, *The Afrikaner’s Interpretation of South African History*, 7. Compare Tiryakian, “Apartheid and Religion,” 392: “In the interior of South Africa, consequently, the Calvinistic doctrine of predestination combined with a belief in the curse on the children of Ham led to an interesting dichotomization of the world into two major groups. The elect of God could be identified by their white skins and by their profession of the

Such, then, are the major theses of the Calvinist paradigm of Afrikaner history and politics. Much of the interest and attractiveness of the paradigm obviously derives from the historical explanation it provides for modern Afrikaner nationalism and the ideology of apartheid.²⁹ Perhaps because of its relevance to contemporary Afrikaner nationalist politics and ideology, most of the literature that uses the paradigm to explain the present in terms of the past derives from the period since the early 1930s, particularly since the 1950s. But the political exploitation of a historical tradition inevitably makes the latter suspect. The case for the validity of the Calvinist paradigm must stand or fall on the nature and function of Trekboer religion on the frontier.

THE MYTH OF THE CALVINIST ORIGIN OF AFRIKANER RACIAL IDEOLOGY has its own history, but not one based on reports of travelers and other contemporary observers. The Calvinist paradigm—including its central tenets—is conspicuously absent from these accounts of the views and attitudes of early Afrikaners until at least the middle of the nineteenth century. And the standard modern accounts propounding the Calvinist paradigm of early Afrikaner racial ideologies—with one apparent exception—do not, in fact, make use of any evidence from the many contemporary descriptions of early Afrikaner views to document the central Calvinist thesis of a popular belief among Afrikaners that they were a Chosen People justified in conquering and subordinating blacks.

The one apparently convincing instance of early Afrikaner use of this notion was documented by Fredrickson. Frontier Afrikaners of the early nineteenth century, he argued, possessed “a body of folk beliefs,” which were inspired by “an Old Testament Christianity of an attenuated Calvinist origin that constituted a prime source of group identity,” well suited to a religious legitimation of white supremacy. After citing instances of belief in the curse on the sons of Ham and the like, Fredrickson continued,

It was no giant step from such beliefs . . . to the notion that the Boers were a chosen people, analogous to the ancient Israelites, who had a special and exclusive relationship with God and a mandate to smite the heathen. A Moravian missionary who visited the Cape in 1815 listened to a settler discoursing at length “on the state of the Hottentots and Caffres, whom he considered as the Canaanites of this land, destined to be destroyed by the white people.”³⁰

In context, the statement appears clear and plausible enough, and the original even continued to characterize the whites as “*the Israelites of God*.”

On closer examination, however, a rather different picture emerges. The passage

Christian Faith; the Damned by their black skins and by their paganism.” For the curse of Ham, see G. D. Scholtz, “Die Ontstaan en Wese van die Suid-Afrikaanse Rassepatroon,” *Tydskrif vir Rasse-aangeleenthede*, 9 (1958): 147–48, and *Die Ontwikkeling van die politieke Denke van die Afrikaner*, 216; Moodie, *The Rise of Afrikanerdom*, 29; MacCrone, *Race Attitudes in South Africa*, 128–34; van Jaarsveld, *The Afrikaner's Interpretation of South African History*, 9; Moolman, “Die Boer se Siening,” 158; and Tiryakian, “Apartheid and Religion,” 392.

²⁹ See, for example, MacCrone, *Race Attitudes in South Africa*, 136: “The history of those times is the politics of the present day.” Also see Crijns, *Race Relations and Race Attitudes in South Africa*, 51.

³⁰ Fredrickson, *White Supremacy*, 170–71.

Fredrickson quoted comes from C. I. Latrobe's *Journal of a Visit to South Africa in 1815* and relates an incident that occurred not on the distant eastern frontier but near the end of his journey back to Cape Town, while he stayed with a miller called Bruckman (probably a German settler) on the Cape Flats. Latrobe, who invariably referred courteously to his various Afrikaner hosts and interlocutors as "Mr. Slabbert," "Mr. Cloete," or "Mr. Delport," identified this particular spokesman merely as "a loquacious countryman . . . who treated us with his opinions."³¹ Elsewhere in the *Journal* Latrobe, who was very conscious of differences in nationality, referred to the frontier farmers and other Afrikaner colonists as "farmers," "Dutch," "boors," and, on occasion, "Africaner." Nowhere did he refer to Afrikaners, collectively or individually, as "countrymen" or "a countryman." Whenever he used the term "countryman," he clearly meant someone of recent European extraction, belonging to the same country of origin. In short, Latrobe's spokesman, the person who propounded these beliefs in the whites as a Chosen People, was not an early Afrikaner but, in all probability, a German compatriot of the miller Bruckman (or, less likely, a British compatriot of Latrobe himself). Significantly, Latrobe compared the views of this "loquacious countryman" not to what he had heard from the frontier farmers on his recent journey but to the notions of "many wicked fanatics in North America, about seventy years ago." Correctly understood, nothing in this passage bears on the folk beliefs of early Afrikaners or supports the view that they subscribed to similar views of a Chosen People.

The documentation usually adduced to support the alleged early Afrikaner tradition to invoke the curse on the sons of Ham as a justification for racial inequality has as its *locus classicus* a passage in the journal of Governor Janssens in 1803. One "brother of Thomas Ferreira," claiming some acquaintance with literature, "made the discovery" that the Hottentots were the descendants of Ham and subject to the curse. Thomas Ferreira's brother certainly succeeded in making an impression on the enlightened and august company, for others recorded his views as well. Modern historians have been equally impressed. G. D. Scholtz, for example, has repeatedly referred to this passage, adding in one instance that the brother of Thomas Ferreira was not alone—that probably the vast majority of Afrikaners shared these views.³² Scholtz did not provide any evidence for this statement, a rather unfortunate omission since this appears to be the *only* documented case of such beliefs among early Afrikaners until the latter half of the nineteenth century! Sometimes a letter from the Drakenstein Church Council to the Convocation of Amsterdam in 1703 is also cited in this connection, although it

³¹ Latrobe, *Journal of a Visit to South Africa in 1815* (London, 1818), 295. For examples of Latrobe's use of "countrymen," see *ibid.*, 132, 218, 249, 255.

³² George McCall Theal, ed., *Belangrijke historiese Dokumenten*, 3 vols. (Cape Town, 1896–1911), 3: 219; and Scholtz, "Die Ontstaan en Wese van die Suid-Afrikaanse Rassepatroon," 147, and *Die Ontwikkeling van die Politieke Denke van die Afrikaner*, 217, 395, 403. Thomas Ferreira headed a clan that figured largely in the complaints of mistreatment and atrocities on Khoi provided by Dr. van der Kemp and others; see Theal, *Belangrijke historiese Dokumenten*, 3: 225–28; and W. B. E. Paravicini di Capelli, *Reize in de Binnenlanden van Zuid-Afrika* (1803; Van Riebeeck Society [hereafter, VRS] edn., Cape Town, 1965), 62–70, esp. 62 n. 270. For reference to the Ferreira statement similar to (and based on?) that of Janssens, see A. van Pallandt, *General Remarks on the Cape* (1803; reprint edn., Cape Town, 1917), 14. No such reference, however, occurs in the most detailed account of the Ferreira case, that by Paravicini di Capelli.

in fact rejected the curse of Ham and its applications: the council raised the possibility of mission work among the Khoisan "so that the Children of Ham would no longer be the servants of the bondsmen."³³ At most, this evidence shows familiarity with the curse on the sons of Ham at an early stage of Cape history.

In any case, however, this notion is by no means specific to or characteristic of a Calvinistic tradition. Its origins can be traced to medieval Jewish writings, and it gained some currency as an explanation of racial color differences in the sixteenth century.³⁴ Early anthropological speculation on the origins of cultural diversity sometimes adduced these notions. In his *Omnium gentium mores* of 1520, Johan Boemus argued that all barbarous peoples were descended from Ham. According to Winthrop D. Jordan, in the eighteenth century "that old idea of Ham's curse floated ethereally about the [North American] colonies without anyone's seeming to attach great importance to it; one Anglican minister asserted that Negroes were indeed descended from Ham upon whom had fallen Noah's curse of slavery, but much more often the idea was mentioned by anti-slavery advocates for purposes of refutation." In Dutch colonial circles of the eighteenth century, the position seems to have been very similar, and in South Africa, as Martin Legassick has pointed out, early references tended to occur in the writings of decidedly humanitarian or liberal officials and missionaries who also had a special interest in refuting such views.³⁵

Unlike the theories proposed in the U.S. South during the 1830s,³⁶ none of these instances of the story of Ham's descendants provides a serious or systematic elaboration of the curse to legitimate slavery and racial subordination, nor did these notions gain much popular currency in South Africa before the latter half of the nineteenth century. It is entirely consistent with this general picture that Governor Janssens attributed the views of Thomas Ferreira's brother in 1803 to his alleged acquaintance with "literature" of some kind and that the governor neither introduced the brother as a well-known or influential figure nor claimed that these views were common among the Afrikaner frontiersmen of his time. In short, Ferreira's brother seems to have had more than his fair share of attention as an early Afrikaner political pundit.

Modern accounts do, of course, make extensive use of the early secondary literature to document many of the subsidiary and circumstantial points within the

³³ Church Council of Drakenstein to the Classis of Amsterdam, April 4, 1704, in C. Spoelstra, *Bouwestoffen voor de Geschiedenis der Nederduitsch-Gereformeerde Kerken in Zuid-Afrika*, 1 (Amsterdam, 1906): 34.

³⁴ See Winthrop D. Jordan, *White over Black: American Attitudes toward the Negro, 1550-1812* (Chapel Hill, 1968), 17-20. For the historical background to the curse of Ham, see, more generally, William McKee Evans, "From the Land of Canaan to the Land of Guinea: The Strange Odyssey of the 'Sons of Ham,'" *AHR*, 85 (1980): 15-43.

³⁵ Jordan, *White over Black*, 201; and Legassick, "The Frontier Tradition in South African Historiography," 54. For Boemus, see Margaret T. Hodgen, *Early Anthropology in the Sixteenth and Seventeenth Centuries* (Philadelphia, 1964), 232-40; and, for Dutch colonial views, see P. Geyl, *Geschiedenis van de Nederlandse Stam*, 3 (Amsterdam, 1959): 72-75; G. Schutte, "Zedelijke Verplichting en gezonde Staatkunde: Denken en Doen rondom de Slavernij in Nederland en Koloniën eind 18^e Eeuw," *Documentatieblad Werkgroep 18^e Eeuw*, Nr. 41-42 (1979): 107-09; and J. M. van der Linde, *Het Visioen van Herrnhut en het Apostolaat der Moravische Broeders in Suriname, 1753-1863* (Paramaribo, 1956), 41-44.

³⁶ See Thomas V. Peterson, *Ham and Japhet: The Mythic World of Whites in the Antebellum South* (Metuchen, N.J., 1978), 65.

Calvinist paradigm, such as the prevalence of religious and providential language, the special emphasis on Old Testament theology, the rejection of *gelykstelling* between whites and blacks, the view that blacks were “creatures,” the disregard for their rights, and so forth.³⁷ But evidence from these sources is conspicuously absent from discussions of the Chosen People thesis, the central claim specific to the Calvinist paradigm and differentiating it from rival accounts of early Afrikaner beliefs and values. There is a very good reason for this rather surprising omission: contemporary descriptions of the views and attitudes of early Afrikaners, irrespective of whether they were “sympathetic” or critical in their treatments, tend to have been cast in a quite different conceptual and moral framework in which “Calvinist” notions did not figure at all. The racial views of Afrikaner frontiersmen of this time seem to have been better caught in Governor Janssens’s earlier remark in the same passage: the frontiersmen “call themselves ‘people’ and ‘Christians,’ and the Kaffirs and Hottentots they call ‘heathen,’ and thereby believe themselves entitled to everything.”³⁸ Landdrost Alberti reported similar views from Uitenhage in 1805: “According to the unfortunate notion prevalent here a heathen is not actually human, but at the same time he cannot be classed among the animals. He is therefore a sort of creature not known elsewhere. His word in no ways can be believed, and only by violent measures can he be brought to do good and shun evil.”³⁹ Whether or not this passage contains some echo of the traditional theory of the Great Chain of Being, as Legassick has suggested, the point is that, unlike the curse of Ham, it does not constitute a quasi-scriptural argument that might reflect a theological tradition. These passages, therefore, fit more easily into a different—and non-Calvinist—paradigm of the contemporary secondary literature.

Until the middle of the nineteenth century, travelers and enlightened officials described the views and practices of frontier Afrikaners in terms of what I call the “degeneracy paradigm.” A good introduction to this dominant perspective is provided by Commissary J. A. de Mist, who surveyed the travel descriptions as well

³⁷ The standard accounts based on the Calvinist paradigm are not, of course, false in every detail. They contain much that is common to any history of white settlement in South Africa. In truth, this literature derives a substantial part of its plausibility through its documentation of historical propositions that are neither necessary to nor distinctive of the Calvinist paradigm. But the ready use of pious and providential language, a preference for the Old Testament, a tendency to fundamentalism, and even the bestowal of biblical placenames hardly provide evidence of a specifically Calvinist tradition as distinct from many other variants of Christian religious practice. Note, too, the very restricted and peculiar sense of Calvinism that is invoked for the paradigm. Strikingly, a specifically theological framework is virtually absent. No one would think of giving an account of seventeenth-century Calvinism in the Netherlands or in England without due regard to the highly developed set of religious doctrines that provided the mainstay for political and social action as well. The very terminology of a “primitive” Calvinism essentially serves as a disclaimer, rendering it unnecessary to discuss matters of theological substance. For an effort toward a comparative discussion of Afrikaner “Calvinism,” see my “Puritans in Africa?”

³⁸ Theal, *Belangrijke historische Dokumenten*, 3: 219. As early as 1739, the rebel Estienne Barbier complained that the Company paid too much attention to “the work of unbaptised Hottentots, who know neither salvation nor damnation”; *ibid.*, 1: 2. For the classic statement of the significance of the original “Christian”-“heathen” dichotomy, see MacCrone, *Race Attitudes in South Africa*, 6–10, 41–45, 125–26. But see Giliomee and Elphick, “The Structure of European Domination,” 359–91; and Fredrickson, *White Supremacy*, 120–24.

³⁹ Landdrost Alberti to Governor Janssens, June 12, 1805, Cape Archives, Cape Town, B.R. 68, ff. 280–81, as quoted in J. S. Marais, *Maynier and the First Boer Republic* (Cape Town, 1944), 73 n. 61. It is of some relevance for the prevalence of these notions among early Afrikaners that Moolman, who was otherwise wholly committed to the Calvinist paradigm, reported that he was unable to find any direct instances of the use of “creature” (*schepsel*) for blacks in the Transvaal before 1860; “Die Boer se Siening,” 68.

as official documents in his *Memorandum on the Cape* in 1802. He concluded his *Memorandum* by citing the parting words of Commissioner Sluysken in 1793 on the need to inquire into the behavior of the colonists themselves to understand the causes of the calamitous frontier conflicts:

What individual, unless he has lost all feelings of humanity, would not, with us, be deeply moved were he to realise that *men could be found so debased and inhuman* as to abuse the very rights and privileges with which the Creator has peculiarly favoured them above all their fellow-creatures, by employing their talents in effecting the undoing of these wretched mortals? Who would not shudder with horror were he to pause for a moment and glance at the pitiful picture of innocent people mercilessly despoiled of their corn and cattle, nay, more, of their wives and children, torn from them by *powerful and rapacious neighbours*? . . . Some, *in spite of their consciences*, actually dare to express a doubt as to whether it would be a crime to murder a Kaffir or a Hottentot. *At most, they regard it as a matter of very small moment.*⁴⁰

Although this passage is primarily an expression of moral outrage containing little specific information about actual frontier views or practices, it reveals something about the moral and intellectual framework that contemporary observers brought to their attempts to understand and describe frontier behavior. Significantly, the *Memorandum* does not suggest that frontier outrages could have derived from a specific religious tradition—that is, to frontier notions of a divinely ordained mission based on predestined election. On the contrary, the assumption is simply that deviant frontier views and practices must have resulted from the *absence* of traditional moral and religious constraints. Many contemporary observers believed that frontier conditions allowed the all-too-human lust for power and domination to have free reign. Officials anticipated that the opportunities for unchecked, rapacious acquisition at the expense of weaker peoples would result in a general debasement and a blunting of moral sensitivity. In short, frontiersmen were in danger of becoming a “degenerate” species of European.

Indeed, both enlightened officials at the Cape and the more settled landholders in the vicinity of Cape Town had long been concerned that the Trekboers’ dispersal in the interior would lead to their cultural and moral degeneracy. The Burgher petition of February 17, 1784, speaks of the threat of “a complete bastardization of morals from so primitive a lifestyle in the veld . . . , a completely degenerate nation, which might become just as dangerous for the Colony as the Bushman-Hottentots now are.” And officials with humanitarian concerns believed that cases of “despotic and barbarous treatment” of servile Khoi were due to the opportunities the frontier masters had to indulge in an “unlimited lust for power” far removed from the watchful eye of church and state.⁴¹ Whether or not their apprehensions had any

⁴⁰ [De Mist], *The Memorandum on the Cape of Commissary J. A. de Mist* (1802; VRS edn., Cape Town, 1920), 256–57 (italics added).

⁴¹ Petition of the inhabitants to the Governor and Political Council of the Cape, February 17, 1784, in C. Beyers, *Die Kaapse Patriotte* (Pretoria, 1967), App. E; and F. R. Bresler, testimony at the trial of R. H. Brits, May 28, 1801, Cape Archives, Cape Town, C/J483, ff. 245–47. The petition of the inhabitants was, in fact, initiated and drafted by Hendrik Swellengrebel in the Netherlands, and his journals contain many other expressions of the degeneracy theory; see the journal entry for October 27, 1776, in G. J. Schutte, ed., *Kaapse Reisjournalen van Hendrik Swellengrebel, Jr., 1776–1777* (forthcoming); and Plettenberg to Swellengrebel, May 12, 1780, in G. J. Schutte, ed., *Briefwisseling van Hendrik Swellengrebel, Jr., oor Kaapse Sake. 1778–1792* (VRS edn., Cape Town,

basis in fact, the existence of these fears provided a ready framework for any reports of atrocities or acts of oppression the frontiersmen committed.

The most influential publication by an English traveler of the early nineteenth century, John Barrow's *An Account of Travels into the Interior of Southern Africa in the Years 1797 and 1798* reveals the same concerns. Barrow was particularly struck by the defective industry of Trekboer existence, a mode of life he considered indolent and slothful compared to the lot of the laboring poor in England. His harsh comments on the frontiersmen of Graaff Reinet are well known. He blamed them for the plight of the Khoi, who he described as "in their present condition, perhaps the most wretched of the human race, duped out of their possessions, their country, and finally out of liberty . . . , a state of existence to which that of slavery might bear the comparison of happiness." Barrow feared that the Khoi might be on the way to extinction at the hands of the colonists, but he attributed this cruel treatment not to any notions of a divine mission or mandate on their part. Rather, these were the practices of "an inhuman and unfeeling peasantry, who, having discovered themselves to be removed to too great a distance from the seat of their former government to be awed by its authority, have exercised, in the most wanton and barbarous manner, an absolute power over these poor wretches, reduced to the necessity of depending upon them for a morsel of bread."⁴²

Other travelers, like H. Lichtenstein and George Thompson, may have depicted the character of frontier Afrikaners more sympathetically, sometimes in a deliberate effort to correct Barrow's unfavorable judgment, but the differences in the accounts are those of emphasis; the conceptual framework is the same. Lichtenstein's own judgment of the "rude and perverted ways of thinking" of the unruly citizens of Graaff Reinet at the turn of the nineteenth century essentially proceeded, despite some variation of nuance and detail, from a social diagnosis similar to that of Barrow:

It seemed as if they had been too much accustomed to live without respect for any earthly power to be easily brought back to a due respect for that under which they were now to live. Their reciprocal irreconcilable spirit of discord and enmity towards each other, their wholly perverted ideas of right and wrong, their extravagant notions with regard to liberty, their total want of true religious principles, though making much external profession of piety, their perfect ignorance in short of all social duties, of all social virtues, had placed them in a most unfortunate situation both for themselves and for the government. The total seclusion of the colonists from general intercourse with the world, and with civilised life, their confinement to the little circle of their own families, the easy manner in which the first necessities of our nature are satisfied, are very disadvantageous to them under many points of view; and notwithstanding their simplicity of manners, their general purity of morals, and their ignorance of many of the greater crimes to which the European nations are subject, they appear, taken in the aggregate, even to impartial observers, much rather under an

1982). For the text of Bresler's testimony, see Du Toit and Giliomee, *Afrikaner Political Thought*, 49–50. Also see Cuyler to Caledon, October 25, 1810, cited in M. Streak, *The Afrikaner as Viewed by the English, 1795–1854* (Cape Town, 1974), 62 n. 55.

⁴² Barrow, *An Account of Travels into the Interior of Southern Africa in the Years 1797 and 1798*, 1 (London, 1801): 136, 137. Also see *ibid.*, 73–81; Streak, *The Afrikaner as Viewed by the English*, 5–22; and D. W. de Villiers, *Reisbeskrywings as Bronne vir die Kerkgeskiedskrywing van die N. G. Kerk in Suid-Afrika tot 1853* (Kampen, 1959), 70–82.

unfavourable than under a favourable point of view. . . . But what is most to be deprecated in the character of some among them, is the harshness with which they treat their slaves and Hottentots, and in others, the bitterness and irreconcilable animosity with which they carry on their differences among each other.⁴³

A generation later George Thompson claimed to have provided a more balanced assessment of Trekboer society than either Barrow or Lichtenstein. Be that as it may, he certainly shared their fundamental assumptions:

That the backcountry boors of former times were many of them as savage, indolent, and unprincipled as Mr Barrow has described, cannot be questioned. . . . But even the *Vee-boors* in general have many good and pleasing qualities, and their worst are, in my apprehension, clearly to be ascribed to the many disadvantageous circumstances under which they are placed; to their being thinly scattered over an immense territory, out of the reach of religious instruction, or moral restraint; to the vicious and corrupt character of the old Dutch government, by which the interests of the community were constantly sacrificed for those of the company and its servants; to the inefficient police, which not only allowed but encouraged and abetted a system of unrighteous aggression against the native tribes; and last, not least, to the influence of slavery, which, wherever it prevails, inevitably deteriorates and pollutes the whole mass of society.⁴⁴

Thompson clearly provided a more complex analysis than his predecessors had, but the essential theme of cultural and moral degeneration, caused by the relative absence of traditional social constraints, remained.

In his controversial *Researches in Africa* (1828), Dr. John Philip, a humanitarian with considerable polemical skill, marshaled what historical evidence was available to build a case against the colonists' "work of oppression, slavery and extermination." Philip saw his task as raising "the voice of humanity" against "the clamour of passion and selfishness." He attributed the oppressive practices of the colonists not so much to any peculiar religious traditions or to racial ideologies but in large part to material interests. According to Philip, the commando system and its concomitant evils arose from the colonists' need to find forced labor, and the system fell into disuse when it no longer served this interest. But the colonists had powerful motives for continuing the prevailing oppressive system, and Philip stressed in particular the opportunities the frontier provided for exercise of the "love of uncontrolled authority, one of the strongest passions in the human breast." His views on the barbarizing effects of power unrestrained constitute a classic statement of the degeneracy paradigm. As a general law of human nature, Philip postulated, "it is now universally admitted that it is the natural tendency of the exercise of uncontrolled authority to harden the heart, extinguish the moral sense, and give birth to every species of crimes and calamities." Accordingly, it was only natural that the moral senses of these frontier colonists would be blunted and their humanitarian values eroded, since they could exploit their fellow men unchecked: "The secluded condition of the greater part of the South African farmers, the power thrown into their hands by the weakness of government, their situation in the midst

⁴³ Lichtenstein, *Travels in Southern Africa in the Years 1803, 1804, 1805, and 1806* (1812, 1815; VRS edn., Cape Town, 1928), 463–64.

⁴⁴ Thompson, *Travels and Adventures in Southern Africa*, 2 (London, 1827; VRS edn., Cape Town, 1968): 87.

of a population of slaves and Hottentots over whom they can tyrannize without control, is as unfavourable to the civilization of the farmers themselves, as it is to the happiness and improvement of those under them."⁴⁵ Philip's remarks on the colonists' views of blacks as an inferior race who might be killed "as game or noxious animals" should be understood within this general framework. Significantly, when Philip discussed theological arguments that might have provided a justification for racial oppression, he set his remarks in the context of European thought in works by the likes of Rousseau and Lord Kames; he considered the application of such religious theories to the South African case only in the most generalized way.⁴⁶ Nowhere did he evince any awareness of an indigenous tradition of "primitive Calvinism" on the part of the colonists themselves.

The prevalence of the degeneracy paradigm in the writings of these travellers and missionaries is, of course, primarily indicative of their own mode of thinking. Their comments do not, therefore, provide primary evidence for the beliefs of early Afrikaners. Armed with their own set of assumptions, these writers may not have been able to recognize the evidence for a tradition of "primitive Calvinism" among frontier Afrikaners: a rationale based on a distinctive religious tradition did not fit their preconceived intellectual framework. Nevertheless, their writings remain important sources for the recording and transmission of early Afrikaner views, sources that we, perhaps, might identify as conforming to some different pattern. This same literature also reports more directly early Afrikaner views on issues like conquest and racial subordination, but, once again, recourse to more elaborate legitimations of these practices is conspicuously absent, at least until the 1830s. In discussing such issues as the excesses of the commando system, either the early Afrikaners evinced a callous insensitivity to the blacks' loss of life and land and generally showed no appreciation of black rights, or they readily acknowledged the injustice and cruelties that had been part and parcel of colonial encroachment and frontier conflict.

Typical is Commandant Nel's discussion of the use of commandoes since the 1790s against the San on the northern frontier. "In justification of this barbarous system," Thompson wrote in 1823, Nel "narrated many shocking stories of

⁴⁵ Philip, *Researches in South Africa*, 2 vols. (London, 1828), 2: 33, and 1: xiv, 86, 383. Also see *ibid.*, 1: 53, 58, and 2: 34–37, 310–19, 394. Thomas Pringle shared many of the same views: "I do not consider the Dutch-African colonists as worse than other people would have been in similar circumstances . . . without strong *legal restraints*, such, alas! is human nature on the large scale, that mere humanity will always be too feeble for passion and selfishness"; Pringle, *Narrative of a Residence in South Africa* (London, 1855), 241. Also see J. W. D. Moodie, *Ten Years in South Africa* (London, 1835), 175: "The Dutch at the Cape of Good Hope afford an instance of a people partially relapsed into barbarism from want of education, and from their intercourse with a race of savages whom they have subjected and demoralized; retaining most of the vices of Europeans, with the cruelty of the slave-holder and savage." The American missionary Daniel Lindley, who ministered to the Trekkers in Natal for some years, also saw their plight in these terms; see his letters of July 17, 1839, and December 1, 1837, cited in Edwin W. Smith, *The Life and Times of Daniel Lindley* (London, 1949), 159–60, 162–63. Also see R. Edwards to A. Tidman, September 4, 1849, cited in Moolman, "Die Boer se Siening," 60 n. 41. The degeneracy paradigm was still dominant more than thirty years later in the Reverend J. McCarter's account of Afrikaner views and character; see McCarter, *The Dutch Reformed Church in South Africa* (Edinburgh, 1869), 134–43. Stokes cited McCarter's account as supporting the Calvinist paradigm; "Afrikaner Calvinism and Economic Action," 73–74. But such a reading clearly depends on taking the relevant passage totally out of context.

⁴⁶ Philip, *Researches in South Africa*, 1: 53, and 2: 315–16. For other remarks on blacks as an inferior race, which need to be set in this larger context, see *ibid.*, 1: xv, and 2: 31, 37; and Pringle, *Narrative of a Residence in South Africa*, 265.

atrocities committed by the Bushmen upon the colonists—which, together with the continual depredations upon their property, had often called down upon them the full weight of vengeance.” Thompson added a perceptive comment on what he called the “hereditary sentiments of animosity, and the deep-rooted contemptuous prejudices” that in his view had blinded Commandant Nel’s judgment and seared his better feelings on this point:

It struck me as a strange and melancholy trait of human nature, that this Veld-Commandant, in many other points a meritorious, benevolent and clear-sighted man, seemed to be perfectly unconscious that any part of his own proceedings, or those of his countrymen, in their wars with the Bushmen, could awaken my abhorrence. The massacre of many hundreds of these miserable creatures, and the carrying away of their children into servitude, seemed to be considered by him and his companions as things perfectly lawful, just, and necessary, and as meritorious service done to the public, of which they had no more cause to be ashamed, than a brave soldier of having distinguished himself against the enemies of his country: while, on the other hand, he spoke with detestation of the *callousness* of the Bushmen in the commission of robbery and murder upon the Christians; not seeming to be aware that the treatment these persecuted tribes had for ages received from the Christians, might, in their apprehension, justify every excess of malice and revenge that they were able to perpetrate.¹⁷

Significantly, Nel made no attempt to invoke any notion of divinely elect and nonelect races, perhaps because he perceived no need for any such special legitimization.

Likewise, Thomas Pringle quoted at length and to similar effect from a letter by an Afrikaner recounting his discussions with frontiersmen on such topics. The letter concluded, “It has indeed often been my lot to be thrown by accident or otherwise among Cape colonists in their unguarded moments, when with evident sincerity and contrition they have deplored the deeds of blood, both of early and recent times, by which the colony has been polluted.” This attitude is in accord with the judgment of Andries Stockenström, who had vast experience in frontier affairs as landdrost of Graaff Reinet. Writing in his *Autobiography* of his long years in office, he claimed that, in his experience, the older generation of colonists readily admitted to the historical realities of conquest and dispossession: “It never entered the imagination of the simplest of the Boers to deny the oppression, knowing that he could not take a step without crossing ground of which those he holds in bondage were once the free and contented owners. The reflecting part [that is, many] of the old population regretted the evil, but could not see a remedy.” In the same context, Stockenström contrasted these traditional Afrikaner attitudes with mid-century ideological legitimations of conquest and added, “The theory which makes the black irreclaimable savages, fit only to be exterminated, like the wolves, was not of Boer origin.”¹⁸

By the 1830s, the naive responses and unsophisticated attitudes of these earlier generations were no longer possible. The whole climate of colonial thinking had

¹⁷ Thompson, *Travels and Adventures in Southern Africa*, 2: 6–7. Also see *ibid.*, 1: 40.

¹⁸ Pringle, *Narrative of a Residence in South Africa*, 239–40; and [Stockenström] *The Autobiography of Sir Andries Stockenström*, 1 (Cape Town, 1887): 78–79.

been profoundly changed, above all by the controversy surrounding the publication of Philip's *Researches in South Africa* in 1828 and by the much-publicized proceedings in London of the Select Committee on the Aborigines, which highlighted the colonists' violent conquest and continuing exploitation of the original and rightful inhabitants. Such organs of (British) settler opinion as the *Grahamstown Journal* construed these activities as a direct challenge to the very moral and political bases of colonial society. Prior to the 1820s, the general issues of colonial settlement and conquest had been largely unproblematic, even to the few Afrikaner intellectuals at the Cape; as late as 1820, when Christoffel Brand's dissertation on the right of settlement appeared,⁴⁹ he saw little need to provide special justifications for colonial conquest and dispossession. In the 1830s, however, Chief Justice J. A. Truter and Advocate J. de Wet found it necessary to produce rather contrived and quite elaborate theories in support of the contention that the colonists had at no time illegally dispossessed the original inhabitants of the land. The most vehement and influential repudiations of Dr. Philip's and his humanitarian allies' critiques of colonial society undoubtedly came from such spokesmen of the British settlers in the eastern Cape as Robert Godlonton, editor of the *Grahamstown Journal*, one of the most prominent organs of settler opinion.⁵⁰ The Great Trek got under way at the height of this controversy, and the leaders of the Trek could not have had any illusion that violent conquest of the indigenous peoples in the interior and seizure of their land would go unchallenged any longer. Their consciousness that the whole enterprise of the Trek might be jeopardized in this way greatly influenced the public statements and official correspondence of Trekker leaders.

One feature of this controversy has been quite obscured in the heat of the polemical recriminations: Dr. Philip and the other humanitarian critics of colonial society themselves subscribed to definite rationalizations for the spread of empire and of Western civilization. If anything, these humanitarian writings tend to display more highly developed notions of civilizing mission than do any of the colonial justifications. As the Preface to his *Researches in South Africa* makes clear, Philip explicitly started from the premise that the spread of Christianity, the interests of an expanding empire, the growth of free trade and capitalism, and the transition of traditional societies to Western culture must all go together:

While our missionaries, beyond the borders of the colony of the Cape of Good Hope, are everywhere scattering the seeds of civilization, social order, and happiness, they are, by the most unexceptionable means, extending British interests, British influence, and the British empire. Wherever the missionary places his standard among a savage tribe, their prejudices against the colonial government give way; their dependence upon the colony is increased by the creation of artificial wants; confidence is restored; intercourse with the colony is

⁴⁹ Brand, "Concerning the Method and Law of Acquiring Colonies," in his *De Jure Colonialium* (Leyden, 1820). For an edited translation of this chapter from Brand's dissertation, see Du Toit and Giliomee, *Afrikaner Political Thought*, 206–08.

⁵⁰ [Truter] "The Origin and Developments of the Laws of the Colony," *Het Nederduitsch Zuid-Afrikaansche Tijdschrift* (1836); and de Wet, "Something about the So-Called Earlier Right of Ownership to This Country of the Hottentots," *ibid.* (1838). For edited translations of both papers, see Du Toit and Giliomee, *Afrikaner Political Thought*, 210–13. And, on Godlonton, see B. A. LeCordeur, *Robert Godlonton as Architect of Frontier Opinion*, AYB (1959), no. 2; and A. L. Harington, *The Grahamstown Journal and the Great Trek*, AYB, no. 2 (1969).

established; industry, trade and agriculture spring up; and every genuine convert from among them made to the Christian religion becomes an ally and friend of the colonial government.

Philip was under no illusion regarding the effects of this entire process on traditional societies: the ancient customs and the traditional ways of life of the indigenous communities would be disrupted and uprooted, their subsistence economies would be transformed, and established political structures would be done away with. But, he believed, these fundamental alterations to the social fabric represented “the triumphs of reason over ignorance, of civilization over barbarism, and of benevolence over cruelty and oppression.”⁵¹ Other missionaries also argued that the best guarantee for the protection of the interests of the indigenous communities lay, at least in principle, in systematic imperial expansion and missionary endeavor to incorporate and transform the traditional societies. Perhaps the most perceptive and sophisticated statement along these lines is contained in Wesleyan missionary William Boyce’s *Notes on South African Affairs* (1838).⁵² Underlying such arguments, though not explicitly, was some notion of a national and civilizing mission that was ultimately rooted in divine ordination.

Such notions were not peculiar to missionary circles or to Victorian England. In the mid-nineteenth century, ideas of a national mission or destiny—ideas that often served as justifications for or rationalizations of imperialist actions or ambitions—were widely prevalent. The vigorous and explicit doctrine of Manifest Destiny, “a dogma of supreme self-assurance and ambition, that America’s incorporation of all adjacent lands was the virtually inevitable fulfillment of a moral mission delegated to the nation by Providence itself,” flourished in the United States during the Mexican War of the 1840s.⁵³ Not surprisingly, then, foreign observers and commentators occasionally applied these familiar notions to their accounts of the dramatic events of the Great Trek as well. Dutch professor U. G. Lauts, for example, attempting to publicize the lot of his unknown and distant kinsmen in the Netherlands in *De Kaapsche Landverhuizers* (1847), drew quite naturally on Old Testament parallels with Israel and on notions of a special civilizing mission: “Should they perhaps be destined and specially chosen by God’s omnipotent wisdom to bring civilization and Christianity in the midst of Africa’s wilderness, and to let it take root there!”⁵⁴ Such speculations about the possible significance of the Trek tell us more about the intellectual framework of Lauts than about that of the Trekkers, whom he only knew from correspondence and other published accounts.

Much closer to the scene was the French traveler A. Delegorgue, who actually spent some time with the Trekkers in Natal during the momentous years of the early 1840s. In his *Voyage dans l’Afrique australe* (1847), he wrote, “They thought of

⁵¹ Philip, *Researches in South Africa*, 1: ix–x, x.

⁵² Boyce, *Notes on South African Affairs* (Grahamstown, 1838), Notes 7–8, pp. 165–95.

⁵³ Albert K. Weinberg, *Manifest Destiny* (Baltimore, 1955), 1–2.

⁵⁴ Lauts, *De Kaapsche Landverhuizers of Nederlands Afstammelingen in Zuid-Afrika* (Leyden, 1847), 19. Lauts was so much taken with the notion that the Trekkers constituted a continuation of the Dutch struggle in the sixteenth and seventeenth centuries for a “purified Christianity” that he attributed the Trek to a striving for freedom of conscience; *ibid.*, 5, 31.

themselves, these men, as another people chosen by God, before whom, but far off across the desert, opened the promised land, with gates defined by two columns put there by the hand of the Creator.”⁵⁵ This may well be the earliest unambiguous attribution by a first-hand observer of Chosen People ideas to early Afrikaners. But the passage is brief and isolated. It occurs in a relatively speculative section discussing the rights and wrongs of the Trek and of the British government’s response; it is not related at all to the conquest and subordination of indigenous peoples, and it had little or no impact on the subsequent literature. The same cannot be said of the writings of David Livingstone.

THE FAMOUS MISSIONARY DAVID LIVINGSTONE articulated, clearly and explicitly, the theological legitimations of colonization and imperial conquest that are largely implicit in the writings of his contemporaries. More specifically, he was also the first writer who directly attributed specific notions of divine mission to Afrikaners as a historical explanation of their racial views and practices. It was Livingstone who in his *Missionary Travels and Researches in South Africa* (1858) initiated the shift from the degeneracy to the Calvinist paradigm in the literature on early Afrikaner politics and history. Notions of calling and divine mission always loomed large in Livingstone’s life, and he naturally cast his work on the opening up of the African interior and, in a different way, his quarrels with Afrikaner leaders in such terms. In so doing, he almost singlehandedly reshaped everyone’s perceptions of early Afrikaner history.

Like Dr. Philip and other missionaries before him, Livingstone clearly assumed that the objectives of Christian mission, the promotion of commerce, imperial expansion, and the general diffusion of the “blessings of civilisation” were intimately and providentially linked. In taking this position, Livingstone no doubt reflected the instinctive Victorian assumptions about expansion, which Ronald Robinson and John Gallagher have described as “suffused with a vivid sense of superiority and self-righteousness, if with every good intention.”⁵⁶ But Livingstone also articulated an explicitly theological legitimation of colonization, which includes the superior right of more civilized peoples to settle territory inefficiently occupied by nomadic peoples or hunting communities. Invoking the “Divine Charter” of Genesis 1:28 that the whole earth is given to man for his cultivation, Livingstone wrote,

Such being the charter on which all primitive lands may be held, it seems plain that the man who subdues or cultivates a portion of the earth has a better title to it than he who only hunts over it. He bestows his labour upon it, and that is his property. Viewed in the light of the Divine and primitive charter, the rights of a civilised community, willing to toil the soil, are

⁵⁵ Delegorgue, *Voyage dans l’Afrique australe notamment dans le territoire de Natal*, 2 (Paris, 1847): 100. (I am indebted to Professor Leonard M. Thompson for this reference.) Delegorgue may have been influenced by some of the statements in the correspondence of the Natal Volksraad, such as the letter to Governor Napier of January 14, 1841; for the letter, see H. S. Pretorius, ed., *Voortrekker-Argiefstukke, 1829–1849* (Pretoria, 1949), 117–20. Such expressions utilizing notions of a civilizing mission in justification of the Trek, however, need to be interpreted in the context of the diplomatic and polemical concerns of this correspondence; see Du Toit and Giliomee, *Afrikaner Political Thought*, 199–202.

⁵⁶ Livingstone, *Missionary Travels and Researches in South Africa* (New York, 1858), 34; and Robinson and Gallagher, *Africa and the Victorians: The Climax of Imperialism in the Dark Continent* (London, 1961), 2–3.

superior to those of savages who derive a precarious subsistence from roots and wild beasts; because the former are willing to enter into the Divine design to render the earth productive of the greatest amount of good to the greatest possible number, and no man has a right to perpetuate a wilderness in any part of the world if his brother man needs it for subsistence.⁵⁷

Even though Livingstone went on to say that the claims even of savages must be held sacred, he only meant that they should be properly compensated when dispossessed of their land. He clearly believed in a divine mandate for European colonization.⁵⁸

Livingstone described his concept of the "Divine Charter" for colonization as the "orthodox" position. That notion did indeed have a long intellectual history. Its proponents included New England Puritans like John Winthrop and the eighteenth-century authority on natural law doctrine Emer de Vattel. And it had provided an important legitimation for those who supported the removal of the Indian tribes from Georgia in the 1820s.⁵⁹ But Livingstone was the one who first applied it specifically to Africa. Because of the charter's emphasis on cultivation and the spread of commercial enterprise, however, Livingstone could use it to justify his own efforts in opening up a route into the heart of Africa but to deny this legitimation to the Trekker settlements in the Transvaal. Livingstone stressed that the Transvaal Boers were not agriculturalists but, like the blacks themselves, nomadic and extensive cattle farmers. Their settlements in the interior did not serve either to promote more intensive cultivation of the soil or to draw these territories directly into an expanding market economy. Thus the Boers' claim to the land, unlike the claims of other European colonists, were not superior to those of the blacks. Afrikaner settlement in the Transvaal, in other words, lacked the divine legitimation of the mainstream of Western colonization, of which Livingstone was a prime agent: "The encroachments of the Boers differ essentially from those of the Americans and other civilised communities, inasmuch as they cultivate less of the soil than do the aborigines whom they expel. Indeed, it is not land they seek to appropriate so much as cattle and slaves."⁶⁰

Yet from the start these very Afrikaner communities proved a major obstacle to Livingstone's missionary endeavors among the Tswanas and threatened to obstruct his efforts to open up the "Road to the North." That threat became more serious

⁵⁷ Livingstone, "The Transvaal Boers and Slavery," in I. Schapera, ed., *David Livingstone: South African Papers, 1849–1853* (VRS edn., Cape Town, 1974), 76.

⁵⁸ *Ibid.*, 77. Also see the telling terms in which Livingstone depicted the indigenous peoples of Africa in connection with his own mission: "No one can conceive the terrible depravity to which the children of Ham have sunk unless he has intimate knowledge of their language and customs, but to change their savage nature is the object of our mission and it will be accomplished by the power of God's spirit through the gospel"; [Livingstone] *Livingstone's Private Journals, 1851–3*, ed. I. Schapera (Berkeley and Los Angeles, 1960), 161.

⁵⁹ See Weinberg, *Manifest Destiny*, chap. 3. Weinberg cited John Winthrop's *Conclusions for the Plantation in New England* and Emer de Vattel's *The Law of Nations* (London, 1760), Bk. 1.

⁶⁰ Livingstone, "The Transvaal Boers," 77. Some thirty years later, perhaps under Livingstone's influence, Sir Bartle Frere had a different view of the Boers' right to conquest: "The Boers had force of their own, and every right to conquest; but they had also what they seriously believed to be a higher title, in the old commands they found in parts of their Bible to exterminate the Gentiles, and take their land in possession. We may freely admit that they misinterpreted the text, and were utterly mistaken in its application. But they had at least a sincere belief in the Divine authority for what they did, and therefore a far higher title than the Zulus could claim for all they acquired." Minute of November 16, 1878, quoted in F. E. Colenso and E. Durnford, *A History of the Zulu War* (London, 1880), 135.

following the recognition of the political independence of the Transvaal Republic by the Sand River Convention of 1852. When Livingstone wrote in his *Missionary Travels* that “the Boers resolved to shut up the interior, and I determined to open the country, and we shall see who have been the most successful in resolution, they or I,” he referred to more than a purely practical conflict.⁶¹ To Livingstone, Boer obstruction represented a direct challenge to a divinely ordained, civilizing mission by a people who themselves distinctly lacked any such legitimation. Such conflicts involving his own concepts of divine mission undoubtedly informed the terms in which Livingstone came to describe early Afrikaner views and history.

Livingstone could easily have couched his descriptions of the views and practices of the early Afrikaners he came to know in the Transvaal in terms of the degeneracy paradigm. His earliest accounts, published pseudonymously in the *British Banner* in 1849, do contain some expressions along these lines. Livingstone stressed the Transvaal Boers’ belief in their racial superiority as the underlying explanation of their violent commando excesses and their capture of blacks to be used as labor apprentices. “Very few of those, therefore,” Livingstone wrote, “who entertain the above opinions . . . think, when they shoot a native, they murder a man.” But he still attributed these racial prejudices primarily to the moral degeneracy brought about by frontier conditions: others “would find it difficult to realise the depth to which the standard of right and wrong has sunk. The moral tone is, indeed, fearfully low.” He concluded this paper, however, with a pregnant observation that tends to put his earlier statements in a rather different perspective: “The idea, however, is nearly universal among them, that they are the peculiar favourites of Heaven, —that they occupy exactly the same position in the Divine favour as the Israelites who were led through the Red Sea; and that this Potgieter [*sic*] is, notwithstanding his Satanic tricks, a second Moses.”⁶² This is perhaps the earliest use of the notion that Afrikaners were a Chosen People as a putative legitimation for racial inequality. Although this concept had not yet become the guiding principle of Livingstone’s account (it was thrown in almost as an after-thought without any attempt to generalize it as the key to understanding the whole of Afrikaner history), Livingstone here began to lay the foundation for what became the Calvinist paradigm.

It is difficult to know with any certainty to what extent Livingstone’s initial observation of Afrikaner identification with a Chosen People was, or was not, a faithful reflection of first-hand encounters with the ideas of Transvaal settlers in the 1840s. During the early years of his work among the Bakwena of Sechele, he made several journeys into the Transvaal and had some opportunity to study the Transvaal Boers at close quarters. But Livingstone’s relationships with the Trekker leaders, particularly with Andries Potgieter, were never easy and open. Livingstone admitted to his lack of interest in early Afrikaner ideas and activities except insofar as they related to his own work: Afrikaner “politics, except so far as they operated on the natives and our Mission, never occupied my attention.” Even before he had had any direct dealings with the Trekker communities in the Transvaal, he had

⁶¹ Livingstone, *Missionary Travels and Researches in South Africa*, 45.

⁶² [Livingstone] *British Banner*, November 14, 1849, in Schapera, *South African Papers, 1849–1853*, 19–20.

been firmly persuaded that the Boers were his adversaries, incorrigibly bent on enslaving the indigenous peoples. "As the Boers themselves wish to be slave holders it is easy to see we must be their determined opponents," he wrote in 1843. "We shall be obliged to do our utmost to oppose their unrighteous ways, or lose all influence over the natives. With the Boers we can do nothing. There are some pious people among them, but others are so self-righteous they will not be taught."⁶³

Livingstone was not, moreover, averse to manipulating some of his material in a good cause. In this case, Livingstone wrote to a friend that his papers in the *British Banner* had deliberately concentrated on the "horrible" aspects of the Transvaal Boers' policy, describing them in language that could be "thought overcharged." And Livingstone was prone to generalize from the particular to the Boers' disadvantage: in a private letter of 1846, for example, he recounted that one Boer had told him he might as well teach the baboons as the blacks; in the *British Banner* paper of 1849 this had become the opinion of "not a few of these vagrants"; and still later he attributed this view to the Boers generally who "always" retracted it when challenged to test their own literacy with that of a Bechuana.⁶⁴ A particular incident, perhaps an encounter with the sect of the *Jerusalem-gangers* under Commandant Enslin, may have provided the basis for Livingstone's initial observation on the new Israelites.⁶⁵ But, without independent confirmation from contemporary primary sources of early Afrikaner thinking, the informative value of Livingstone's statement is suspect. Whether or not Livingstone's account accurately reflects early Afrikaner opinion, however, his penchant for generalization is manifest. And, between 1848 and 1852, his initial and fairly casual observations coalesced into the central category of Livingstone's later account, not only for the views and practices of the Transvaal Boers but for Afrikaner history in its entirety as a result of a series of further controversial encounters with Afrikaner leaders.⁶⁶

In December 1848, during his last trip into the Transvaal, where he had planned to station a black missionary teacher near Rustenburg, Livingstone had a confrontation with Potgieter. Trekker leaders were deeply suspicious of the political motives of the missionaries from the London Missionary Society (LMS), and Potgieter, advised by the Hollander H. T. Bührman, alleged that Livingstone intended to

⁶³ Livingstone to William Ashton, April 6, 1849, reproduced in Schapera, *South African Papers, 1849–1853*, 23–24, and Livingstone to Henry Drummond, July 29, 1843, quoted in Schapera, *South African Papers, 1849–1853*, Preface.

⁶⁴ Livingstone to D. W. Watt, January 13, 1850, in Schapera, *South African Papers, 1849–1853*, 2, Livingstone to Pyne, December 1846, as quoted *ibid.*, 19 n. 16, and "The Transvaal Boers and Slavery," *ibid.*, 85. Also see Livingstone, *Missionary Travels and Researches in South Africa*, 37–38; and Livingstone to his parents and sisters, July 28, 1850, in I. Schapera, ed., *David Livingstone: Family Letters, 1841–1856*, 2 (London, 1959): 94.

⁶⁵ Enslin's fanatical views impressed other contemporary observers on the scene, such as James Chapman, although, significantly, he did not mention any religious justification in use by Transvaal leaders for the commando attack against Sechele and its aftermath. And Chapman was in a much better position than Livingstone to observe the debates among the leaders. See Chapman, *Travels in the Interior of South Africa, 1849–1863* (reprint edn., Cape Town, 1971), 85–88. Also see *ibid.*, 20, 82; Andrew Murray's letters, cited in J. du Plessis, *The Life of Andrew Murray* (London, 1919), 103, 123; and Spoelstra, *Die Doppers in Suid-Afrika, 1760–1899*, 115–18. For a more detailed study, see J. P. Claasen, "Die Jerusalem-gangers: Met besonderse verwysing na J. A. Enslin" (M.A. dissertation, University of South Africa, 1978).

⁶⁶ The following account is largely based on the meticulous analysis provided by Schapera in his edition of Livingstone's *South African Papers, 1849–1853*.

occupy the land on behalf of the British government. Livingstone replied “that if [Potgieter] hindered the gospel the guilt of the loss of many souls would rest on his head.”⁶⁷ Soon thereafter, Livingstone heard that a small deputation from the Dutch Reformed Synod in the Cape—consisting of Dr. W. Robertson and the Reverend P. E. Faure—was touring the Transvaal and planned to hold a service on the nearby farm of Commandant Gert Kruger, where (it turned out) Andries Pretorius was a guest. Making use of the opportunity, Livingstone asked Robertson and Faure to use their influence with the Trekker leadership to intercede on behalf of his missionary objectives. At first, Livingstone found Dr. Robertson “very friendly” and willing. What happened next became a matter of considerable dispute. According to some observers, and confirmed by Robertson and Faure’s own later account, the deputation found the Commandants not opposed to missionary work as such but hostile to Livingstone personally because, the Trekkers alleged, he had supplied the Bakwenas with arms that they had then used against a rival tribe. The deputation then proposed a meeting with Livingstone at which the charges against him could be aired, but, although he agreed, Livingstone departed before the appointed time following an angry altercation with some of the other farmers.⁶⁸

Livingstone’s own account is somewhat different. He made no mention of the proposed interview and claimed that Potgieter and Kruger had prevailed upon him to postpone his project at Rustenburg with a view to a forthcoming popular meeting at which they promised to use their influence on his behalf. Instead, on January 23, 1849, Potgieter sent a letter, drafted by Bühman, to the Secretary of the LMS District Committee demanding Livingstone’s recall from his mission at Kolobeng, failing which his expulsion was threatened. Livingstone vigorously denied the contents of “Potgieter’s Boerish Bull,” which included such charges as inciting the Bakwena to defy the supremacy of Trekker rule, which the tribe had earlier acknowledged, and fleeing “hastily” from the meeting where he was supposed to have answered the allegations that he had supplied arms to the tribesmen. Evidently, Livingstone was particularly riled by the “enormous fib” that he had cowardly run away.⁶⁹

Although the threats to expel him were not at first carried out, the “slanders” about his conduct continued to circulate, and Livingstone believed that Dr. Robertson in particular actively spread malicious gossip about him. Robertson had apparently recounted the “gun story” to Robert Moffat, Jr., before realizing that he was Livingstone’s brother-in-law. Henceforth, Livingstone closely identified his Transvaal adversaries (“Dr. Robertson’s converts”) and “the Reverend Slanderer.”⁷⁰

⁶⁷ Livingstone to Robert Moffat, January 18, 1849, in Schapera, *David Livingstone: Family Letters, 1841–1856*, 9.

⁶⁸ John Kotze, Letters to the Editor: “The Trans-Vaal Farmers and Secheli,” *Cape Town Mail*, April 19 and 23, 1853; Robertson and Faure, Letter to the Editor, *ibid.*, June 4, 1853. Both letters have been reprinted in Schapera, *South African Papers, 1849–1853*, 26–27, 34–35.

⁶⁹ See [Livingstone] *Private Journals, 1851–53*, 302–03; Livingstone to Arthur Tidman, May 26, 1849, in I. Schapera, ed., *Livingstone’s Missionary Correspondence, 1841–1856* (Berkeley and Los Angeles, 1961), 128–29; Livingstone to Robert Moffat, January 18, 1849, in Schapera, *Family Letters, 1841–56*, 10–11. Also see Schapera, *South African Papers, 1849–1853*, 21–25, 35; and, for the text of Potgieter’s letter, see *ibid.*, App. 1, pp. 126–29.

⁷⁰ Livingstone to William Thompson, September 6, 1852, in Schapera, *Missionary Correspondence, 1841–1856*, 216; and Schapera, *South African Papers, 1849–1853*, 33. For Robertson’s encounter with Moffat, see *ibid.*, 32.

The link was further strengthened when in June 1852 Robertson published a long defense of the Transvaal farmers, claiming that the deputation “did not discover the smallest vestige” of slavery among them. Livingstone, who was in Cape Town at the time, prepared a rebuttal containing his own version of the origins of the charges that he had supplied arms to the Bakwena, a rebuttal that was subsequently published in the *Cape Town Mail* as “The Story of the Black Pot.”⁷¹ While Livingstone was still in Cape Town, Dr. Robertson also figured prominently in the celebration launched under the auspices of the Synodical Commission of the Dutch Reformed Church of the bicentenary of van Riebeeck’s settlement at the Cape as a “*Religious Festival* in commemoration of the Introduction of Christianity into South Africa.”⁷²

In August 1852, not long after the Sand River Convention had granted territorial recognition to the Transvaal Republic, a commando unit under Commandant P. E. Scholtz moved against Sechele and defeated the Bakwena. During the attack, Livingstone’s premises at nearby Kolobeng were broken into and plundered. Two other LMS missionaries, Walter Inglis and Rogers Edwards, wrote a letter to Scholtz to protest against the actions of the commandos, and particularly against their capture of young blacks. Indenturing blacks as labor apprentices, in the missionaries’ view, amounted to enslavement, which was precluded by the terms of the Sand River Convention. The two missionaries published their allegations in newspapers abroad, and, in retaliation, they were charged with libelling the republican authorities, tried before a court in Rustenburg, and expelled from the Transvaal. According to a statement Inglis made some months later, he used his appearance in court to confront Transvaal leaders, including Commandant Scholtz and Commandant-General Pretorius, with the moral issues involved in the conduct of the commandos and the use of forced labor. In reply, Inglis claimed, Commandant Scholtz had justified their conduct by invoking “the Divine law given to Joshua.”⁷³ To the missionaries, his response amounted to claiming a divine mandate for the Transvaal Boers’ conquest and subjugation of the blacks in a new system of slavery. The commando raid against Sechele and the expulsion of the missionaries received considerable publicity in South African newspapers, and a heated controversy ensued.

Livingstone was at Kuruman on his way to Kolobeng when he received news of the commando attack. Although he had already resolved to give up his mission among the Bakwena (and, in fact, regarded this commando as a divine judgment on the tribe for resisting the Gospel), he was deeply hurt and incensed by the “sack of Kolobeng.” Subsequent events did nothing to soften his views. Forced to delay his projected journey to the interior for several months, he wrote formal letters of

⁷¹ *Cape Monitor*, June 2, 1852; and *Cape Town Mail*, April 26, 1853, in Schapera, *South African Papers*, 1849–1853, 27–33.

⁷² Robertson, Letter to the Editor, *Cape Monitor*, June 2, 1852, cited in Schapera, *South African Papers*, 1849–1853, 73 n. 41.

⁷³ Inglis, Statement in *Bloemfontein Gazette*, January 20, 1853, reprinted in the Parliamentary Blue Book, *Further Correspondence Relative to the State of the Orange River Territory*, Accounts and Papers, 46 (1852–53): 120–22. For a discussion of the probable meaning of Scholtz’s invocation of “the divine law given to Joshua,” see my “Captive to the Nationalist Paradigm.”

protest to British officials and sent impassioned accounts of the destruction wrought by the commando to his correspondents at home and abroad. He also wrote two papers intended for the *British Quarterly Review*, but only published posthumously, on "The Transvaal Boers and Slavery."⁷⁴ In these papers, Livingstone linked the various issues and agencies involved in the complex controversy of 1848–52 in a way that, taken as a whole, suggested an entirely new perspective on Afrikaner history. And the papers provide background and depth of detail essential to understanding Livingstone's briefer treatment of the Transvaal Boers' views and practices in his *Missionary Travels*.

Were it not for the mid-century controversy, the central theme of Livingstone's papers on "The Transvaal Boers" might be unintelligible. For Livingstone attacked the Dutch Reformed Church as the ideological source of persistent injustice to blacks throughout the entire course of Afrikaner history. In a private letter, he stated that the object of his remarks was to show "that their church is, and always has been, the great bulwark of slavery, cattle-lifting, and Caffre-marauding."⁷⁵ Holding the Cape Dutch Reformed Church responsible for the actions of the Trekker communities seems, at first glance, somewhat farfetched, since the Transvaal Boers had at that point been denied any formal affiliation and had had, in fact, minimal contact with the Cape Church for almost fifteen years. But, Livingstone suggested, the link between the church and the Boers should be understood in a broad historical perspective of a peculiar, particular religious tradition—and, in so doing, he produced the earliest statement of the Calvinist paradigm of Afrikaner history.

Livingstone began his argument by pointing to the religious character of the Afrikaner founding fathers: "They expatriated themselves in order to enjoy the privilege of worshipping God according to the dictates of their consciences. In this respect they bore a strong resemblance to the Pilgrim Fathers of New England. They were undoubtedly men of God." In this depiction of the character of the first settlers, Livingstone concurred with the picture of the early Afrikaners espoused by the promoters of the van Riebeeck bicentenary. But Livingstone proceeded to link this tradition to the legitimization of racial inequality and oppression:

Having done and suffered much for their religion, they naturally felt that in comparison with the Hottentots, who knew little of the Great Spirit, they were the peculiar favourites of Heaven. Some appropriated to themselves in a temporal sense Divine promises only meant to be fulfilled to the Church or its Head in a spiritual sense. They concluded, for instance, that the heathen were given *to them* for an inheritance, and the uttermost parts of the earth for possession.

This short passage outlines the main theses of the Calvinist paradigm, and Livingstone attempted to apply them to the whole of Afrikaner history. He alleged that van Riebeeck's *Journal* shows evidence of what he termed "*the Van Riebeeck*

⁷⁴ The essays were eventually published in the *Catholic Presbyterian*, 2 (1879): 412–23 and in William G. Blaikie, *Life of Livingstone* (2d edn., London, 1881), App. 6, pp. 490–511. The Blaikie version is reprinted in Schapera, *South African Papers, 1849–1853*, 70–95.

⁷⁵ Livingstone to Watt, October 3, 1853, as quoted in Schapera, *South African Papers, 1849–1853*, 67, from Blaikie, *Life of Livingstone*, 128.

principle,” which served to legitimate the theft of the indigenous peoples’ property whenever practicable. In Livingstone’s view, the van Riebeeck principle had been “a prominent feature in the history of the border Boers during the last two hundred years,” and it had, moreover, been implicitly condoned by the Dutch Reformed Church, which had “for centuries uttered no protest against the Van Riebeeck principle or practice” and had not censured the guilty parties in such “robbery and murder, provided the victims had black skins.”⁷⁶

Within this historical framework, Livingstone adduced the familiar evidence regarding the linguistic usage of “men” and “Christians” exclusively for whites and of “creatures,” “folk,” and “black property” for the indigenous peoples, so that these beliefs in racial superiority could appear to result from a particular religious tradition. Similarly, the beliefs of that Transvaal sect that thought of itself as literally on the way to Canaan acquired a special significance and, in Livingstone’s analysis, embodied the central thrust of Afrikaner thinking and history: “The religious element, as might be expected from those who are traditionally Christian, enters largely into their vagaries. Many of the Potgeiter [*sic*] party believe that they, as the chosen people of God, are travelling to the land of Canaan. They were, a few years ago, by no means backward in stating their belief that Potgeiter was a second Moses.”⁷⁷ In this connection Livingstone represented Commandant Scholtz’s invocation of the “Divine law of Joshua” at the trial of Edwards and Inglis as a direct scriptural legitimation of what he described as “a bloody slave-hunt”: “On their return from killing upwards of one hundred adults, and while actually driving more than two hundred children to and from the waters along their route as a flock of goats, they quoted Deut. XX, 10–14 as containing a full justification of all they had done.” The difference between the Afrikaners and the New England Puritans was to be found, Livingstone concluded, in the endorsement of the “Van Riebeeck principle”—legitimizing racial conquest and slavery—by the Dutch Reformed Church.⁷⁸

In comparison to this comprehensive and explicit treatment of Afrikaner history in “The Transvaal Boers and Slavery,” Livingstone’s discussion in Chapter 2 of his *Missionary Travels* is both very brief and quite restrained. But the chapter contains all of the essential theses of the Calvinist paradigm in the following brief passage, which constitutes Livingstone’s explanation for early Afrikaner involvement in so many “bloody scenes” of conquest and establishment of “a new species of slavery” in the republics:

They [the Boers] are all traditionally religious, tracing their descent from some of the best men (Huguenots and Dutch) the world ever saw. Hence they claim to themselves the title of

⁷⁶ Livingstone, “The Transvaal Boers,” in Schapera, *South African Papers, 1849–1853*, 72–74.

⁷⁷ *Ibid.*, 84. Also see *ibid.*, 78. Livingstone apparently linked the Afrikaners and the Jews in another, more profane sense. His private journal for June 1852 commented on the roguery, lack of truthfulness, and materialism of the Afrikaners he had met on the way from Cape Town, and he concluded, “They are more like Jews than any people I know. They have respect for religion but not enough to be religious themselves.” *Private Journals, 1851–3*, 84. Not long afterwards, he wrote to William Thompson that “the Boers are certainly the remnants of the Lost Tribes of Israel. They speak of nothing but pounds, shillings, dollars, guilders, sheep and oxen. Their whole souls seem absorbed by this world’s goods. Their talk is just exactly what you would overhear in the Jews of London.” Schapera, *Missionary Correspondence, 1841–1856*, 212.

⁷⁸ Livingstone, “The Transvaal Boers,” in Schapera, *South African Papers, 1849–1853*, 85, 94. Also see *ibid.*, 86.

"Christians," and all the colored race are "black property" or "creatures." They being the chosen people of God, the heathen are given to them for an inheritance, and they are the rod of divine vengeance on the heathen, as were the Jews of old.⁷⁹

Owing to his immense prestige as an African explorer, especially following the acclaim of his Cambridge lectures in 1858, Livingstone's *Missionary Travels*, and this passage in particular, became the single most influential source of stereotypes for early Afrikaner history and thought.⁸⁰

THE INFLUENCE OF LIVINGSTONE'S NEW PARADIGM can be traced through much of the secondary literature on Afrikaner history and politics during the second half of the nineteenth century, a literature that in turn came to be regarded as authoritative. John MacKenzie, for example, cast his account of the frontier Afrikaners in the Transvaal, *Ten Years North of the Orange River* (1871), generally in terms of the degeneracy paradigm. But, when he came to such familiar items in the descriptive literature as the frontiersmen's familiarity with and preference for the Old Testament, the linguistic usage of "heathen" and "Christian," the prominent function of religious observances even on commando, and the like, MacKenzie shifted his ground. "No one who has freely and for years mingled with this people can doubt," he wrote, "that they have persuaded themselves by some wonderful mental process that they are God's chosen people, and that the blacks are the wicked and condemned Canaanites over whose heads the Divine anger lowers continually."⁸¹ MacKenzie's brief characterization has become one of the most frequently cited sources in the later literature.

MacKenzie's account seems to provide strong support for the Calvinist paradigm. But MacKenzie was careful enough to present the crucial theses as inferences from

⁷⁹ Livingstone, *Missionary Travels and Researches in South Africa*, 37. Also see *ibid.*, 36.

⁸⁰ When surveying the literature on Transvaal history in 1896, the American writer W. E. Garrett Fisher still referred to Livingstone's work as "an authority on the question"—specifically in discussing the explanation for the Transvaal Boers' "union of strong religious feeling with extreme brutality to the natives"; Fisher, *The Transvaal and the Boers* (New York, 1896), 18–19. There is only one early statement consistent with the Calvinist paradigm that cannot be traced directly to the published works of Livingstone, but it originated in the same historical context and the writer had concerns very similar to those of Livingstone. See the appendix to William C. Holden, *History of the Colony of Natal* (1855), which deals with the history and politics of the Orange River Sovereignty. In discussing the principles of "Boer native policy," Holden wrote, "They are a religious people in their way, read the Bible, and profess to be regulated by its precepts; but, in reference to slavery, they look on the dark-skinned races as the 'cursed sons of Cain,' who are doomed to perpetual servitude. Therefore they consider themselves as performing the will of God, and doing Him service, when they inflict this cruel wrong on the Natives." *Ibid.*, 390. Holden does not seem to have had much direct intercourse with the Afrikaners of the interior. He did follow the controversy regarding the commando raid against the Sechele very closely, publishing extensive documentation on it. His purposes were frankly polemical: the main object of his long appendix was to forestall, if possible, granting independence to the Orange Free State, which he believed would only compound the error that had been made in the Transvaal. This section of Holden's work seemingly reflects missionary interpretations of Commandant Scholtz's appeal to the law of Joshua at the trial of English and Edwards; elsewhere in his book, at any rate, Holden consistently described the Afrikaner pioneers of the interior in terms of the degeneracy paradigm. Compared to Livingstone's, Holden's influence was slight, although Theal may have used him; see note 82, below.

⁸¹ J. MacKenzie, *Ten Years North of the Orange River* (Edinburgh, 1871), 50–51. Also see *ibid.*, 48–50, 52–55. W. D. MacKenzie quoted this passage at length, and his quotation is also often cited; see W. D. MacKenzie, *South Africa: Its History, Heroes, and War* (1899), 157–59. For an account based on personal experience of racial views and practices in the Transvaal, an account that includes some elements of the Chosen People construct but does not attribute them to a "Calvinist" tradition, see P. Huet, *Het Lot der Zwaarden in Transvaal* (Utrecht, 1869).

otherwise varied evidence. He did not claim that he had ever heard anyone actually maintain the notion of a Chosen People as legitimization of conquest and subordination; he only argued that this must be the explanation for various other views and practices. MacKenzie himself did not mingle “freely and for years” with the Transvaal Boers and, in fact, seems to have had fairly limited and superficial contacts with them. He was, however, greatly impressed by Livingstone’s example and was undoubtedly familiar with *Missionary Travels*. Thus MacKenzie’s paradigmatic conclusions regarding the primary evidence at his disposal were, in all probability, rather the result of his reading of Livingstone than of any direct observations of his own.

A second, and equally important, example of Livingstone’s influence concerns no one less than George McCall Theal, founding father of South African historiography. In his early *Compendium of South African History and Geography* (1876), Theal included a brief chapter on the history of the South African Republic. After a paragraph based on Livingstone’s description of the Sechele raid and its aftermath and a rather uncharacteristically purple passage on the death of Andries Pretorius (“Incessant mental labour had told upon an iron constitution”), Theal evaluated Pretorius’s character and career:

Thus died Andries W. J. Pretorius, the ablest leader and most perfect representative of the Emigrant Farmers. To get a key to his character, it is necessary to go back thirty-three centuries in the world’s history, and to set up for an ideal a Caleb or a Joshua. Born and brought up in a land where slavery was tolerated, accustomed from infancy to hear of the native races only as of depredators, inheriting a feud towards them which the massacre of near relatives had bitterly intensified, Pretorius could hardly have been expected to fashion his native policy in accordance with modern English ideas. But there was another and stronger feeling at work, which was the cause of such acts as the attack upon the Bakwene and numerous exploits of a like nature. The Emigrants believed that they were the chosen people of God, and that the heathen were given into their hands for an inheritance. There can be no doubt that Pretorius thought he was acting in accordance with divine command in treating the natives as he did. The injunctions given in the twentieth chapter of Deuteronomy were therefore followed to the letter.⁸²

It is difficult not to detect the influence of Livingstone’s paradigm, which can hardly be coincidental. Theal even quoted the entire passage from Deuteronomy used by Livingstone, although there are no specific grounds to link it with Pretorius.

Theal’s account was, in turn, the direct source of Anthony Trollope’s speculations in his influential *South Africa* (1878), which fleshes out some of the main theses of the Calvinist paradigm. Trollope presented, on Theal’s authority, the influence of the Old Testament and the Afrikaner belief in having a mandate similar to that of Israel to conquer and enslave the heathen peoples as the key to pioneer attitudes toward forced labor:

In studying the peculiarity of the Dutch character in South Africa and the aversion of the people to our ways we have always to remember that they had been brought up for ages in

⁸² Theal, *Compendium of South African History and Geography* (Lovedale, 1876), 163–64. Theal may have used Holden’s *History of the Colony of Natal*, which contains extensive material on the death of Pretorius, including the official letter written by Landdrost Lombard. For the letter, see *ibid.*, 392–94.

the strictest belief in the letter of scripture. . . . It was so two hundred years ago with a large sect in Europe—from which sect they had sprung. They had grown in the new land without admixture with the progressing ideas of Europe. They had neither been enlightened nor contaminated by new systems of belief, or unbelief. So it has come to pass that an institution which is so abhorrent to us as to make us feel that the man who is stained by it must be a godless sinner, is still to them a condition of things directly authorized and ordered by the Almighty.

A few pages later, Trollope reflected on the legitimization of conquest and colonization, making it clear that modern Europeans also assumed some divine mission or mandate, albeit different from that attributed to the early Afrikaners:

I venture to express an opinion that to all just men, who have turned this matter in their thoughts with painful anxiety, there has come a solution—which has by no means satisfied them, but which has been the only solution possible—that God Almighty has intended that it should be as it is. The increasing populations of the civilized world have been compelled to find for themselves new homes; and that they should make these homes in the lands occupied by people whose power of enjoying them has been very limited, seems to have been arranged—by Destiny. That is the excuse we make for ourselves; and if we do not find verbal authority for it in Deuteronomy as do the Boers, we think that we collect a general authority from the manifested intention of the Creator.⁸³

Although Theal, whose work Trollope used, omitted both his account of Pretorius's death and his speculative interpretation of the leader's significance from his subsequent and more considered treatments of this period, his early analysis based on Livingstone's writings was repeatedly cited as a supposed source for the Calvinist paradigm.

Theal also made a second contribution to the sources of the Calvinist paradigm in the literature, but in this instance both the influence of Livingstone and the paradigmatic quality of the passage are slight and indirect. In discussing the character of colonial society at the end of the eighteenth century, he provided a summary and commentary on the earlier secondary literature of South African history. Theal was much concerned to correct the possible bias in the descriptions of the colonists' views and activities by such travelers as Barrow:

Naturally the life of the graziers beyond the coast plateau visible from Table Mountain was ruder than that of the more settled corn and wine farmers, though it would be incorrect to attribute this to anything like stupidity on their part, for they were capable, under changed circumstances, of living very differently. . . . They were simply Europeans of an advanced type who had adapted themselves to a rough environment.

Theal believed that travelers who dismissed the colonists as taciturn and uncommunicative and ridiculed them as ignorant of and not interested in European affairs hardly did the pioneers of the interior justice:

Those travellers . . . would probably have been surprised if an aged grazier had spoken about the lives of any of the old testament heroes or had compared a storm on the lofty inland

⁸³ Trollope, *South Africa*, 2 (London, 1878; reprint edn., Cape Town, 1973): 275, 281–82. Also see Trollope's interesting reflections on the historical significance of the Kimberley mines as a primary "civilizing" agent compared to the efforts of Christian missionaries; in this case, however, he did not introduce a theological dimension. *Ibid.*, 368–69.

plains with that grand description of one given in the twenty-ninth psalm. On subjects like these the grazier could talk freely enough, because they came within the range of his experience. He was living under such skies as those under which Abraham lived, his occupation was the same, he understood the imagery of the Hebrew writers more perfectly than any one in Europe could understand it for it spoke to him of his daily life. He had heard the continuous roll of thunder which was the voice of the Lord upon many waters, and had seen the affrighted antelopes drop their young as they fled before the storm, when the great trees came down with a crash and the lightning divided like flames of fire. He knew too of skies like brass and of earth like iron, of little clouds seemingly no larger than a man's hand presaging a deluge of rain, and of swarms of locusts before whose track was the garden of the Lord, while behind was a naked desert. When he spoke of these things he could be eloquent enough, but they were not subjects for conversation with casual visitors.⁸⁴

This remarkable passage, which deserves inclusion in any anthology of South African historical writing, is really nothing more than imaginative historical reconstruction, a purely speculative enterprise: he tried to steer a course between the degeneracy and the Calvinist paradigm. But the former had become outmoded, and the latter had become commonplace.

After the late 1870s, Livingstone's new paradigm had largely replaced Barrow's model of the cultural gap. Historians and political commentators at the end of the nineteenth century—John Noble, A. Wilmot, J. MacKinnon, Lord Bryce, Archibald R. Colquhoun, Leroy H. Hooker, and others—wrote of South Africa, the Afrikaners, and their views and racial policies entirely within the new framework. The most consistent articulation of the Calvinist paradigm and a virtual compendium of like-minded accounts (although, unfortunately, the sources are not always identified), appears in W. E. G. Fisher's *The Transvaal and the Boers* (1896), and the book clearly reflects Livingstone's influence.⁸⁵ On the whole these writers provided minor variations on the general theme, showing familiarity with the conventional literature rather than indicating any specific knowledge of primary sources in support of the main theses of the paradigm.

J. F. van Oordt, the contemporary biographer of President Kruger and, therefore, a writer more qualified than most to be regarded as an authority on Afrikaner views, repeatedly and confidently asserted such main theses of the Calvinist paradigm as the ideology of a Chosen People, the association of the Trek with the exodus to the promised land, the Calvinist origins of the founding fathers, the divine mandate that delivered even the mightiest indigenous tribes to the Trekkers, and the continued presence of God at work in the historical struggle of the Republics.

However much van Oordt was in a position to know what he was talking about, his impressive and influential *Paul Kruger en de Opkomst van de Zuid-Afrikaansche Republiek* (1898) is no more specific about sources for these assertions than is any other representative of the contemporary literature with which he was evidently

⁸⁴ Theal, *History of South Africa before 1795*, 4 (London, 1922; reprint edn., Cape Town, 1964): 367, 369–70.

⁸⁵ See Noble, *South Africa: Past and Present* (London, 1877), 169; Wilmot, *The Story of the Expansion of South Africa* (London, 1895), 162–63; Bryce, *Impressions of South Africa* (London, 1898), 142–43; MacKinnon, *South African Traits* (Edinburgh, 1887), 223–85; Hooker, *The Afrikaners* (Chicago, 1900), 125–26; Colquhoun, *The Africander Land* (London, 1906), 212; and Fisher, *The Transvaal and the Boers*, 15, 18–21, 79.

familiar. Nowhere did van Oordt either document them or claim he knew from personal experience that such views had been or were still prevalent. All of his statements within the Calvinist framework are cast in the most general terms, as a model or standard against which to judge specific actions. Thus, if Kruger's theocratic notions were an anachronism in the enlightened nineteenth century, he argued, then that was a judgment about the defects of the contemporary world, not the deficiencies of Kruger's. Although van Oordt seems to have been intrigued by the notion of a cultural time-lag across several centuries, it is not clear that he took it as a serious intellectual proposition: when he reproduced the text of one of Kruger's Paardekraal addresses, he asked his readers if they, in reading the speech, had felt transported from the materialistic age to the Calvinist Europe of two centuries earlier, and the nature and tenor of the question seem to indicate that the author was not comfortable—and did not expect his readers to be comfortable—with Kruger's rhetorical idiom.⁸⁶ Yet van Oordt, like so many of his contemporaries, found it appropriate to invoke just such remote "Calvinistic" origins to make sense of what was, after all, recent political history.

By 1900, THE STORY BECOMES QUITE COMPLEX. At this point, the historical myth that had been generated in the secondary literature about Afrikaner history caught up with its *ex post facto* appropriation by Afrikaners themselves and its regeneration within neo-Calvinist and nationalist notions. More often than not, the process was abetted by relative "outsiders" who operated as spokesmen for the Afrikaner community with its indigenous traditions and emerging historical consciousness. Van Oordt, for example, was not simply a "young Afrikaner intellectual," but a resident of recent Dutch extraction and influenced by his university studies in the Netherlands. Kruger was congenial subject matter, for he had his roots deep in the Trekker tradition, particularly in the conservative Doppe subcommunity that had itself been reconstituted in a distinct Afrikaner church (the *Gereformeerde Kerk*) under the aegis of ministers from the *Afgescheide* Reformed Church in the Netherlands, the spiritual home of Abraham Kuyper. Kuyper's neo-Calvinist program was propagated, with little success, by an Afrikaner nationalist, the Reverend S. J. du Toit during the 1880s.

The intellectual idiom of Du Toit's neo-Calvinism was as unfamiliar to the Afrikaner mainstream as was, though in a different way, Kruger's more archaic and idiosyncratic rhetoric in his Paardekraal speeches.⁸⁷ Yet in this context emerged the first authentic, if nascent, articulations of an Afrikaner Chosen People ideology.⁸⁸ And it is here that the actual history of modern Afrikaner "Calvinist" notions, culminating in Dr. Verwoerd's ideological vision of an apartheid order as the divine mission of Afrikanerdom, should start. As has been the case in other nationalist

⁸⁶ Van Oordt, *Paul Kruger*, 3, 27, 28, 185, 559–60, 807–08, 814–15.

⁸⁷ For a comprehensive and critical survey, including Kruger's speeches and S. J. du Toit's early writings, see my "Puritans in Africa?"

⁸⁸ See, for example, C. P. Bezuidenhout, *De Geschiedenis van het Afrikaansch Geslacht van 1688 tot 1882* (Bloemfontein, 1883); Du Toit, "This Is God's Finger?" *De Getuige*, 1 (1881): 49; and *Hehemia als Volkshervormer* (Paarl, 1885). Also see the editorial in *De Zuid Afrikaan*, October 13, 1885.

nationalist movements as well, Afrikaner nationalism is less the product of its unique cultural roots than the result of the ideological labors of a modernizing elite seeking to ensure social cohesion in transitional times.⁸⁹ The historical myth of a primitive Afrikaner Calvinism is a quite different story. As long as this nineteenth-century myth serves to give spurious historical weight to twentieth-century social ideology, both the practice of history and the practice of politics will suffer.

⁸⁹ See, for example, Robert M. Berdahl, "New Thoughts on German Nationalism," *AHR*, 77 (1972): 65–80; and G. Eley, "Nationalism and Social History," *Social History*, 6 (1981): 83–107.



Research Note
Once More: Pearl Harbor, Microdots,
and J. Edgar Hoover

LETTERS AND REPLIES

Research Note is a new feature of the *AHR* in which the editors intend to publish short articles on recent archival discoveries or other revelations of major historical significance. In the first edition (December 1982) we published John F. Bratzel and Leslie B. Rout, Jr.'s "Pearl Harbor, Microdots, and J. Edgar Hoover" (87: 1342–51). To our surprise, this essay was the subject of scores of newspaper articles, radio broadcasts, and television commentaries on three continents. But it also attracted a rebuttal from the Federal Bureau of Investigation, which was released to the press days ahead of its receipt by the editors of the *AHR* and the article's authors, to whom it was officially addressed. Ordinarily, the exchange of letters that followed would have been printed in the Communications section of this issue of the *Review*. The editors believe, however, that the new issues raised by that correspondence are of such importance to historians generally that they should be aired in this edition of *Research Note*. THE EDITOR

TO THE EDITOR:

I am writing concerning the article by Michigan State University historians John F. Bratzel and Leslie B. Rout, Jr., entitled "Pearl Harbor, Microdots, and J. Edgar Hoover." We have delayed commenting on this article until the completion of processing relevant FBI documents under the Freedom of Information Act. These documents have now been processed for release, and copies are attached.*

It is our belief that the enclosed materials effectively refute the allegations in the article that former FBI Director J. Edgar Hoover withheld pertinent information on Pearl Harbor furnished to the FBI by Popov from other U.S. intelligence

* Copies of the documents mentioned throughout can be obtained from the Federal Bureau of Investigation. For a reproduction of the telegram containing the microdots, of J. Edgar Hoover's letter to presidential aide Major General Edwin M. Watson, and of part of the text (with the FBI translation) of the microdot material, see Bratzel and Rout, "Research Note—Pearl Harbor, Microdots, and J. Edgar Hoover," *AHR*, 87 (1982): 1344, 1346, 1348. THE EDITOR.

agencies. As the documents indicate, information contained in the questionnaire, including that on Pearl Harbor, was paraphrased by the FBI and furnished to military and naval intelligence offices for their use in providing us data to serve as disinformation for transmittal to the Germans through Popov.

For your information, in September 1981 Mr. Bratzel wrote to this office inquiring about Mr. Hoover's sending only a portion of the questionnaire to the White House and asking for general information on Popov and his activities during World War II. Mr. Bratzel's request for general information on Popov was referred to the FBI's Freedom of Information/Privacy Acts Section for processing. In response to his specific questions, this office advised him that from the context of Mr. Hoover's letter of September 3, 1941, to the White House, it appeared that a copy of one of the items making up the questionnaire was being sent only to illustrate the mircrodot as a significant German espionage technique. Bratzel was further told the FBI had sent similar communications to the assistant secretary of war and the undersecretary of the navy but that, at the same time, the FBI, in discharging its internal security responsibilities, was in contact with both the military and naval intelligence offices concerning the substantive messages in the questionnaire.

In response to Mr. Bratzel's subsequent inquiry for further specifics into the matter, this office suggested to him that he await the FBI's processing of his earlier request for material under the Freedom of Information Act, as that material would in all likelihood be pertinent to his inquiry. Unfortunately, Mr. Bratzel did not wait for the documents to be released before he and Mr. Rout finished their article. It should also be pointed out that the report of the commission to investigate the Pearl Harbor disaster, headed by Associate Justice Owen J. Roberts, contains a wealth of detailed information on Japanese interest in Pearl Harbor available to military and naval officials well before December 7, 1941.

Based on the above information, we believe that the comments in the article authored by historians Bratzel and Leslie B. Rout, Jr., constitute an inaccurate and unfair attack on Mr. Hoover and the men and women of the FBI, and we would appreciate your bringing the contents of this letter to the attention of your readers. I have written to Mr. Bratzel (with copies of that letter and documents to Professor Rout) concerning the comments in the article and have advised him that we are also writing to the *American Historical Review*.

ROGER S. YOUNG

*Assistant Director in Charge,
Office of Congressional and Public Affairs,
Federal Bureau of Investigation
March 30, 1983*

PROFESSORS BRATZEL AND ROUT REPLY:

When the *American Historical Review* agreed to publish our article, we expected that there would be some publicity, and there was. Since the subject is volatile, we also expected some crank mail; we were not disappointed. A number of letters,

including one written on a shirt cardboard, accused us of defaming “the last true anti-Communist,” while another set of letters lauded us for proving that Hoover was a dastardly villain. Our favorite was from a man in Colorado who congratulated us for proving Franklin Roosevelt was a Communist in league with Joseph Stalin.

Although we enjoyed reading these letters, it became clear to us that the writers had read into the article (or newspaper accounts of it) their own interpretations. They adapted our words to satisfy their prejudices. As noted, we expected some of this, but we were amazed when the FBI sent us a letter, dated March 30, 1983, which charged that our article constituted “an inaccurate and unfair attack on Mr. Hoover and the men and women of the FBI.” Obviously, some at the FBI thought they were reading a partisan attack rather than a scholarly article.

First, we did not “attack . . . Mr. Hoover.” Nowhere did we argue that Hoover was responsible for the successful Japanese attack on Pearl Harbor, nor did we even suggest that, had Hoover been more sagacious in handling the microdot material, the Hawaiian assault would have been averted. Moreover, we never mentioned “the men and women of the FBI” at all.

What we did say is that Hoover exercised poor judgment when he used the information supplied to him by a British double agent (Dusko Popov) to score bureaucratic points with the assistant secretary of war, the undersecretary of the navy, and the president of the United States. In his report to them, Hoover emphasized the success of the FBI in detecting the microdots rather than the substance and significance of the message they contained. Perhaps more importantly, he did not inform the president or the Office of Naval Intelligence (ONI) or the Military Intelligence Division (MID) that the Japanese were asking probing questions about Hawaiian military and naval installations.

In its letter, the FBI notes that we should have waited for material it was “processing” to be sent to us before we went into print. While doing the research for this article, we mailed two letters to the FBI requesting clarification and additional information regarding the activities of Popov and the disposition of the questionnaire he brought. The first request was answered in six weeks (September 3 to October 15, 1981), the second in three weeks (October 22 to November 13, 1981). We expected the additional material promised in the FBI letter of November 13 (from Roger S. Young) to arrive within a similar period of time. Instead, we waited thirteen months, and nothing appeared. Not until three months after the *Research Note* was published in the *American Historical Review* did the FBI respond, fully sixteen months after our last request. Moreover, the documents seem to have been chosen to respond to the perceived “attack” rather than in answer to our request.

The conclusion we drew—that the questionnaire was not passed to the ONI and MID—was not based on intuition. If the FBI had supplied the Popov questionnaire to the other intelligence agencies, there should be some record of these transactions. We wrote the appropriate archival sources: Old Army and Navy Branch, National Archives (October 22, 1981); the Naval Historical Center, Department of the Navy (October 22, 1981); and the Modern Military Branch, National Archives (October 28, 1981). According to the archivists, no record of Popov’s warning—or, for that matter, anything regarding the questionnaire—was found. Clearly, we had a solid foundation on which to base our assertion.

For historians, the most important charge the FBI made was that our “comments” were “inaccurate.” Roger Young states that “information contained in the questionnaire, including that on Pearl Harbor, was paraphrased by the FBI and furnished to military and naval intelligence offices for their use in providing . . . data to serve as disinformation for transmittal to the Germans through Popov.” As evidence, the FBI placed in the public domain twenty pages of documents. Apparently bolstering the FBI case is a memo signed by R. G. Fletcher, dated September 30, 1941, of a telephone conversation with Popov’s controller, C. F. Lanman, in which the latter states that the “questionnaire . . . had been paraphrased and furnished to the representatives of ONI and G-2 [MID] by Special Agent [A. M.] Thurston.” The operative word here is “furnished,” and it is only in the subsequent reports that its definition becomes clear.

In the case of MID, a memo dated October 1, 1941, provides the definition. Thurston reported that he met with Colonel J. T. Bissell of MID and “requested detailed information of the type indicated in the attached questionnaire.” The second page of the October 1 document contains a paraphrased and abbreviated version of the original Popov material. It deals solely with army matters, and only the first four of twenty lines refer to Hawaii, specifically the dispositions at “Wicham [*sic*] Field and Wheeler Field.” This page might have been the subject of the meeting, but exactly what was discussed will probably never be known. The only clue is that none of the “disinformation” subsequently supplied by MID to the FBI dealt with Hawaii. Nor does the follow-up memo of October 3, 1941, make any reference to the islands.

The case of ONI is similar. An October 20, 1941 memo signed by R. G. Fletcher and sent to Mr. D. M. Ladd informs us that Special Agent Thurston “rephrased and discussed . . . the entire questionnaire furnished [by] Popov concerning Naval matters” with that agency. Attached as page two of the document is a shortened, paraphrased version of the questionnaire, more than half of which deals with naval installations in Hawaii, including Pearl Harbor. Again, what was “discussed” is unclear, but the subsequent “disinformation” supplied by ONI, like that by MID, makes no reference at all to Pearl Harbor.

The documents that the FBI has now finally supplied us seem to confirm our impression, as reported in our article, that J. Edgar Hoover, Special Agent Thurston, and the other agents involved in handling Popov did not conclude from the contents of the double agent’s questionnaire that the Germans were showing an extraordinary interest in Hawaii and its defenses, that the questions they asked probably stemmed from the Japanese, and that those questions might easily have had an operational purpose. Instead of providing ONI and MID with the original questionnaire, the FBI contented itself with two paraphrases, one of which (that sent to MID) seriously obscured the document’s particular interest in Hawaii and Pearl Harbor. To our mind, not even the paraphrase sent to ONI carries the full force and impact of the original document with its repeated requests for information on and, if possible, sketches showing the “exact” location of specific facilities at Pearl Harbor and in Hawaii.

The questionnaire seems to have been handled in a purely routine way, as just one more move in the continuing game of espionage and counterespionage rather than as a vital clue to the purposes of a potential enemy. The FBI's only concern—as revealed in these new documents—appears to have been how to obtain from ONI and MID realistic “disinformation” to be sent back to Berlin through Britain's double agent. While the new documentation shows that the FBI did not ignore the Popov questionnaire, it actually confirms the impression we tried to convey in our article that no one at the FBI evaluated the document for its revelation of German (that is, Japanese) intentions.

This impression is bolstered by another, more recent search of the archives. We sent the twenty pages newly released by the FBI to the three archival depositories noted earlier and requested anything at all pertaining to the FBI representations to MID and ONI (Old Army and Navy Branch, May 11, 1983; Naval Historical Center, April 26, 1983; and Modern Military Branch, April 22, 1983). Once again, the archivists found nothing. No information is available that suggests that ONI and MID were cognizant of the implications of the microdot questions. It appears that the FBI contacted the other intelligence agencies primarily for help in drafting fictitious “disinformation” with which to deceive the Germans, not to supply those agencies with intelligence information that might have alerted them concerning Japanese intentions.

The main thrust of our essay, furthermore, was aimed not at what the FBI sent ONI and MID concerning the Popov questionnaire but at what the agency sent to the White House. Here the new material provided by the FBI tends to confirm our impression that J. Edgar Hoover sent to the White House *only* the relatively harmless part of the questionnaire. In his eagerness to demonstrate to President Roosevelt the efficiency of the FBI, Hoover sent him merely a “curiosity piece”—evidence of the cleverness of the FBI in detecting the microdots. (Actually, it required no cleverness to find and transcribe the microdot message, for the double agent Popov had revealed the secret on his arrival in New York.) Given Roosevelt's naval background and interest in naval affairs, the president might have found the entire questionnaire significant. But we will never know, for he never had the chance.

Finally, it is unfortunate that the FBI seems not to understand the role of the historian. Our goal is not to erect or dismantle images; it is to examine the historical record and draw logical conclusions based on evidence. That is what we tried to do in our *Research Note*, and that is what we believe we accomplished. Perhaps the FBI should consider hiring professional historians to search its files for documents requested by scholars under the Freedom of Information Act. If this task were performed by persons more familiar with what historians seek, the result might be more satisfactory not only to historians but also the FBI.

JOHN F. BRATZEL
LESLIE B. ROUT, JR.
Michigan State University
May 27, 1983



TO MR. TOLAND:

In your recent book *Infamy: Pearl Harbor and Its Aftermath* (1982) you wrote on page 260, note 4, "there are no available records in the FBI concerning Popov's questionnaire. Hoover's second in command, Edward Tamm, never heard of it, but if Hoover had received such information, he told the author, he would certainly have passed it on to Roosevelt." May I ask whether during your research for the book you wrote to the FBI and asked for documentation on the Popov questionnaire? I assume that you did and that the FBI responded negatively. Or did you rely on the statement of Edward Tamm?

I am writing for this information because in December 1982 the *Review* published an article by John F. Bratzel and Leslie B. Rout, Jr., entitled "Pearl Harbor, Microdots, and J. Edgar Hoover." In it the authors relied on a statement from the FBI that the agency could find no document showing that the full questionnaire had been submitted to the president and on letters from various military archives indicating that no material relative to the questionnaire could be found in their files. Although the FBI promised the authors material on the Popov questionnaire, nothing came until eighteen months after the original request and three months after publication of the article.

In a letter to the editor, dated March 30, 1983, Roger S. Young, Assistant Director in Charge, Office of Congressional and Public Affairs of the FBI, has taken issue with the article and provided copies of some documents for the period September–November 1941, which show that the FBI was engaged with the Office of Naval Intelligence and the Military Intelligence Division in an effort to provide the Germans through Popov with false information concerning the defenses at Pearl Harbor. We would like to know whether you also sought this documentation from the FBI and did not receive it.

At issue here is whether the FBI has failed to give you and the authors of the article we published full information concerning the contents of its files on the Popov questionnaire. We would deeply appreciate any information you can give us concerning your experience in this matter. If you corresponded with the FBI on this subject, may we have copies of those letters? If not, can you at least summarize the contents and give us the dates and names of your correspondents? If you wish to see copies of the documents provided us by the FBI in return, I shall be happy to send them to you.

I look forward to hearing from you at your earliest convenience.

OTTO PFLANZE
Editor
June 5, 1983

MR. TOLAND REPLIES:

The history of my dealings with the FBI to obtain information under the Freedom of Information Act is a sorry one. It started in 1978 when I wrote to request *all*

information to be found in the FBI files that might indicate prior knowledge of the coming attack on Pearl Harbor. I received a routine response, dated November 2, 1978, in which I was informed that my application for information would be processed. I suspected that the processing procedure would take too long to enable me to use the material for my book on Pearl Harbor. As a consequence, I proceeded to deal privately with an acquaintance, a former agent of the FBI, in the hope of using his influence to get the material in time for my book. Months passed but nothing came, and he advised me to write for material on specific subjects. Therefore, I wrote on October 11, 1979, requesting specific documents on (1) the Tyler Kent case and (2) Hoover's dealings with Popov.

Mr. David G. Flanders replied on October 26, 1979, that "documents concerning your request on the Japanese attack on Pearl Harbor are currently being processed for disclosure. As you may well understand, documents concerning Pearl Harbor are voluminous in nature and require lengthy review." The review, Flanders continued, required decisions on reclassification and cooperation with various other government agencies, which would take considerable time. In response to a second letter from me requesting specific information, particularly on Popov, Flanders replied on December 12, 1979 that the records concerning "advance knowledge of the Japanese attack on Pearl Harbor" had been processed and were ready for release. They requested \$75.00 in payment for the xeroxing, and I complied on December 16. For this I received on January 10, 1980, 750 pages of useless material. All I managed to salvage was one minor story about midget subs.

On January 17, 1980, I wrote Mr. Flanders again, and on February 4 he responded. They were searching, he said, and would notify me "as soon as possible of the results of this search and the review of any pertinent information." I had received nothing by June 6, 1980, and so I wrote another letter, to which I received no reply. On January 25, 1981, I wrote Mr. Flanders again, this time asking not only for information on Popov but also on Kilsoo Haan and Governor John A. Burns. On February 4, 1981, Thomas H. Bresson responded, "Several voluminous files were located which may contain the information you seek. Due to the specific nature of your request, it is necessary to conduct a page by page review of these files to determine if they contain the information you seek. As this process takes a considerable length of time and due to the large number of requests now being processed, we again solicit your patience and cooperation."

On April 22, 1981, I received good news from James K. Hall. I was to receive information on Tyler Kent, and \$29.30 was required to pay for the copies. On payment of this fee, I received yet another pile of almost useless material. It contained nothing new or revealing. On June 10, 1981, I wrote again. By this time I was getting rebellious, and I asked for a statement on why I was not getting any information on Kilsoo Haan or Popov. On March 11, 1982, when my book was already in print and about to be published, the FBI sent me 228 pages, which contained not one word of what I wanted.

At the end of the covering letter Mr. Hall wrote, "Also, concerning your request for additional information regarding Dusko Popov, John A. Burns, Kilsoo Haan, etc., documents are currently being reviewed and you will be notified if any information is determined releasable." While in Japan in late August 1982 I heard

that two Michigan State historians had received copies of Popov papers from the files of the FBI. So I thought to find on my return to Connecticut in late January 1983 the information I had sought for so long. But I found nothing, despite my long series of requests. Finally, on March 17, 1983, I received a letter requesting \$21.20 to pay for copies of documents of the long-awaited material on Popov, Burns, and Kilsoo Haan. Well, better late than never. Although it was too late to get the information into my paperback of *Infamy* (published in February 1983), I paid the requested sum and received 324 pages. They contained: 1 page on Burns, merely a xerox of a Honolulu newspaper story; a number of letters from Kilsoo Haan; and, finally, a lot of documents on Popov.

Only one of those documents revealed anything about the FBI's handling of the Popov questionnaire—a 4-page “laboratory report,” dated September 3, 1941, telling of the microdots and giving a translation of their contents. Most of the rest were denunciations by the FBI of Popov's claims that he had warned the FBI of an imminent Japanese attack. In a memorandum, dated September 25, 1973, which is typical of the rest of these documents, FBI agent R. R. Franck denounced an article in the approaching October 1973 issue of *True* magazine that claimed, “J. Edgar Hoover could have warned us about Pearl Harbor.” According to Franck, “Nothing in his [Popov's] assignments could have possibly indicated an attack on Pearl Harbor and indeed it is ludicrous to believe the Germans would have sent Popov to the U.S. and asked him to go on to Hawaii to get detailed intelligence information to send back through the Germans to Japan as a basis for an attack by the Japanese on Hawaii.” But the Germans did exactly that.

These documents are all that I received from the FBI concerning Dusko Popov and his role in German-Japanese espionage in late 1941. What the FBI provided the Michigan State historians three months after publication of the *AHR* article they could not find for me during five years of requests, despite my persistent inquiries. I need make no further comment here except to say that I would like to send a message to J. Edgar Hoover, who was indeed most helpful and open with me when I was researching *The Dillinger Days*. I would like to tell him that, despite all the liberal accusations against him, he was the only head of the FBI who was straight with me. “Come back, J. Edgar. We need you.” My relations with the U.S. Navy on Freedom of Information Act materials were just the opposite. They were not only open but helpful and did not even delete the most embarrassing information I revealed about that service.

JOHN TOLAND

Danbury, Connecticut

June 11, 1983

Reviews of Books

GENERAL

EDWARD SHILS. *Tradition*. Chicago: University of Chicago Press. 1981. Pp. viii, 334. \$20.00.

If you admire tradition and traditional ways of thought, you will like this book, since it applauds historical continuity and is an example of what it professes. Edward Shils's work is unusual on the scholarly scene today: it is a conservative critique of the Enlightenment. He thus shares much of the spirit of Ortega y Gasset, Robert Nisbet, and Aleksandr Solzhenitsyn, though his exposition is much more systematic and detailed than theirs; he also has a strange affinity with radical critics of philistinism in mass culture such as Adorno, Benjamin, and Horkheimer.

Shils's great achievement in this book is to have constituted "tradition" as a field of study. With relentless exhaustiveness and great historical learning he analyzes tradition in the West as an organizing concept and discusses variously the role of tradition in social and political institutions, science and technology, literature and the arts, religion and civic literature, and more. In this Shils shows how tradition cannot be eliminated, how it imposes limits on change, and how it is itself subject to inevitable innovations.

Of a total of ten chapters, the first two introduce the concept: distinguish it from history, the past, and related terms; and discuss how tradition operates at various depths in individual and collective beliefs and practices. This is followed by two long descriptive essays on, respectively, the endurance of past objects and past practices. The final third of the volume—seven short, overlapping essays—considers endogenous and exogenous factors in the shaping of traditions, patterns of stability and change, and the history and prospects of the rationalization of societies in relation to tradition.

Shils's tone is sad and reserved rather than polemical and *engagé*. Yet his elegy for tradition is also a critique of social science as well as modern society. In the discursive analysis that he presents, the post-

Enlightenment human sciences appear as fragmented and hyper-rationalistic as, indeed, do modern societies and cultures. By presupposing progress, says Shils, we have denied the role of our traditions and thus buried our own foundations. "A mistake of great historical significance has been made in modern times," he concludes, in the "doctrine which treated traditions as the detritus of the forward movement of society" (p. 330). This of course is not news. What is original to Shils is his having made this perception the inevitable conclusion of a comprehensive discussion of tradition and traditions. And as for solutions? It would be inconsistent with his traditionalist spirit for Shils to suggest more than patient watchfulness and delicate tact in trying "to amalgamate some of the traditions of the Enlightenment and some of those which its heirs have attempted to discard" (p. 330).

All these are strengths of *Tradition*. But the volume also has the flaws of its virtues. In making his points by accumulation, Shils's prose style itself imposes a feeling for tradition. For post-Enlightenment readers, however, this also makes the book irritatingly repetitious and overlong by at least a third. Moreover, the vast material that is presented discursively is left almost entirely untheorized. It is as though "ideographic" is taken to be traditional and "nomothetic" equated with modern, with Shils opting for word pictures instead of general explanations. This gives the volume an integrity of logic and subject, but the amalgamation of old and new styles of thought, which Shils himself calls for, awaits a writer with his historicist and holistic sensibilities, who also can mold logico-deductive theory from the data of Shils's interpretations.

RICHARD HARVEY BROWN
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College Park

JOHN BREUILLY. *Nationalism and the State*. New York: St. Martin's Press. 1982. Pp. x, 421. \$25.00.

John Breuilly believes no general theory of nationalism is possible, and yet, in a way, he has provided

one. He consistently holds that nationalism is a form of politics, a device for pursuing interests that is shaped by, and even created by, the structure of the state system in which it occurs. In its classic form nationalism is a movement in opposition to the modern state. It became possible, therefore, only when a state vested with sovereignty that could be disputed and seized had come into existence. This was particularly true in the empires, where colonial governments, using collaboration with native elites as their basic method of social control, created both the state and the oppositionists who eventually seized that state.

Breuilly systematically criticizes every major school of explanation of nationalism. Postulation of a need for identity is much too general and unprovable, Marxism inevitably retreats to subjective assertions clothed as analysis of objective class interest, modernization theory sometimes ends up sounding a good deal like a nationalist apology, and ideology usually justifies and legitimates political events rather than motivating them. Breuilly's purely political explanation of nationalism does not prevent him from making some interesting points about other things. He agrees that nationalist ideology emerged in part as an effort to refute the ideal of universal reason by showing the uniqueness of each community. But he puts a nice twist on this fact when he suggests that the reason nationalism's rituals and symbols became so powerful is that "nationalists celebrate *themselves* rather than some transcendent reality" (p. 344). For Breuilly, nationalism bridges the gap between politics and nonpolitics, between state and society, by portraying the nation simultaneously as both a political and a cultural entity, a community that should be the state. But the solution is specious. Nationalists switch back and forth between political and cultural justifications as conditions demand, playing on the ambiguity of their imperfectly integrated ideal.

Breuilly's book presents the most fully worked out discussion that we have of nationalism as a structural product of the modern state. For that reason, it may be placed on the shelf beside Kohn and maybe Kedourie (intellectual), Deutsch (communications and nation building), and Gellner (modernization and community), as the strongest statement of a particular view. But Breuilly does not match these classics in overall execution. For example, a good argument could be made concerning the function of nationalism in the Habsburg and Ottoman empires, but misstatements weaken his application of the ideas of legitimation, mobilization, and coordination to this region. And despite occasional striking phrases (for example, "ideology is a means of dealing with puzzlement" [p. 304]), his writing is often tedious and didactic.

The state is being resurrected as an object for

study in several disciplines. Breuilly makes a convincing case that for nationalism too the structural impact of the state was fundamental.

GALE STOKES
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W. WARREN WAGAR. *Terminal Visions: The Literature of Last Things*. Bloomington: Indiana University Press. 1982. Pp. xiii, 241. \$24.50.

The end of the world is in fashion, variously attributed to nuclear war, environmental degradation, or international financial collapse. As we approach the millennial year 2000, more of the same may be anticipated. In *Terminal Visions*, W. Warren Wagar provides a splendid Baedeker for the apocalypse, as it has been envisioned by fiction writers since the beginning of the nineteenth century.

Although the book does indeed deal with "the literature of last things," it would be both unfair and inaccurate to pigeonhole it as merely literary criticism of science fiction. Genre devotees will find much to interest them, no doubt, but the importance of *Terminal Visions* lies elsewhere. For Wagar recognizes that genre literature provides an enormously useful device for tracing secularized religious eschatology, that amorphous set of ideas and images out of which fashionable contemporary cultural pessimism has grown.

Secular eschatology is difficult to unravel. Although the Marxist version has been explicitly and logically developed, non-Marxist apocalypses more often have flourished in the less tended fields of popular culture, and it is to the imaginings of popular culture that Wagar goes. Without slighting *Terminal Visions* as a study of literature, it is first and foremost an essay in the history of ideas, in this case the idea of the end-time. Wagar allows himself the leeway of literary judgments, but quite properly subordinates issues of style, form, and artistic merit to larger questions of world-view and of the sources from which the secular apocalypses flow. Indeed, the author is nowhere more deft than in his demonstrations of the links that bind modern speculative fiction to antecedents in the religious apocalypses of the past.

As might be expected, nineteenth-century and early twentieth-century writers prove easier to treat than contemporaries, where detachment and a full appreciation of cultural context are inevitably harder to come by. This caveat aside, *Terminal Visions* is a major contribution to understanding the current vacillation between millennial hope and *fin-de-siècle* despair, written, one may add, with a wit and verve that go far to counteract the gloom of the subject matter.

MICHAEL BARKUN
Syracuse University

SILVIA ROTA GHIBAUDI. *Lavoro e socialismo: Abbozzo di una storia della concezione socialista del lavoro*. (Saggi e ricerche dell'Istituto di Scienze Politiche "Gioele Solari," Università di Torino, number 33.) Milan: Franco Angeli. 1982. Pp. 156. L. 8,000.

The avowed goal of this book is to sketch out "a history of the socialist conception of work," but it fails to deliver on this promise. Silvia Rota Ghibaudi succeeds rather in providing a short summary of the reflections of major socialist thinkers on the subject. There are attempts to come up to the present, but the book's focus is on the nineteenth and early twentieth centuries. It is certainly useful to have a summary of these speculative reflections gathered in one book that attempts to clarify the evolution of work according to the socialists; but the study is too brief to go into much depth about the subject and does not make an original contribution. Unfortunately, the work almost wholly neglects some schools of thought whose ideas might have proved interesting in this context, for example, the revolutionary syndicalists. Furthermore, Ghibaudi concentrates primarily on French and English thinkers but does not say anything particularly original about them. Although it is a workmanlike book, it is above all a summary whose usefulness will probably be limited to persons who read Italian and are interested in having shorthand references to the ideas of philosophers such as Fourier, Owen, Saint-Simon, Morris, and Proudhon. Some of these ideas do not seem very relevant to her theme, while she misses aspects that could be both interesting and pertinent. Such is the case, for example, with her discussion of the student movement in 1968. Ghibaudi fails to put the movement into context or to investigate its ramifications in France and, more importantly, in Italy.

The book does have positive points, such as the attention the author pays to the role of women in the ideas of the personages she discusses. At times she demonstrates flashes of insight, such as in her division of socialists into "collectivists" and "federalists" and her attempts to explain the responses to reality of different socialist factions in these terms. Her application of this concept helps explain the strength and liveliness of her final chapter, which examines the relationship of work as conceived by socialists to some important social problems.

These strengths, however, do not compensate for the book's overall limitations.

SPENCER M. DISCALA
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BARRY KÄTZ. *Herbert Marcuse and the Art of Liberation: An Intellectual Biography*. London: Verso. 1982. Pp. 234. \$8.50.

One of the most creative interpreters of what Marx really meant, Herbert Marcuse became a national figure of the magnitude of a Senator McCarthy or a Marilyn Monroe by the beginning of 1967. Marcuse was sufficiently discriminating to tell an interviewer, "I'm very much worried about this." But he reflected, "At the same time it is a beautiful verification of my philosophy, which is that in this society everything can be co-opted" (p. 174).

Nearly seventy at the time, Marcuse had earned his ambiguous distinction. He became a Marxist as a young man in Weimar Germany and persisted in claiming that identity while straining the Marxian structures beyond the breaking point. Profoundly influenced by Heidegger's ontology, he had plunged back through Marx's dialectic to take his position on Hegel's dialectic, but as he saw it emended by Freud. Observing American capitalism's cooptation of trade unions, he gave up on the proletariat as another example of the *One-Dimensional Man* (1964). In an article in 1978 he produced a revolutionary replacement in the "universal individual," a *lumpenproletarian* composite of rebellious student, enlightened professional, and liberated woman. He died the next year.

Before that, flattering and liberating, Marcuse had rounded out his life as the active spiritual leader of dissidence in the United States, West Germany, France, and Italy. When it was reaching its peak, he spoke to vast numbers of student rebels at a UCLA teach-in, at a *journée marcusienne* in occupied Nanterre; in Rome, where placards greeted him, "Marx, Mao, Marcuse" (pp. 182, 186); in Berlin, Oslo, Amsterdam, Salzburg, and New York. He knew when to stop and in 1971 told students in Berkeley, "The heroic period of beautiful spontaneity, of personal anti-authoritarianism, of hippie rock and shock, is over" (p. 194).

Barry Kätz has written an effortlessly disciplined intellectual biography that, from Wilhelmine Germany onward, does more than justice to its subject. One would welcome more resonance from the living context—suggestions of the agony in Germany and the rock and carnival music of student protest—but Kätz has kept the promise of his title. The writing is unobtrusively excellent, frequently giving point to Marcuse's more orotund statements. It must be said, however, that Kätz has carried out his assignment as believer, if not as hagiographer, criticizing nothing and rationalizing everything about Marcuse while finding in error, in agreement with his subject, every thinker from Marx to Freud. Thus Kätz has attempted to justify Marcuse's advocacy of repression under cover of the dialectical deception in the phrases "repressive tolerance" and "discriminating tolerance" (p. 172), representing prescribed policy for changing the unjust status quo. But Kätz's sympathy has produced an eminently

worthwhile book (including a Marcuse bibliography) and thereby prepared the way for more critical studies.

DAVID FELIX
Bronx Community College and
Graduate Center,
City University of New York

ANTHONY N. STRANGES. *Electrons and Valence: Development of the Theory, 1900–1925*. College Station: Texas A&M University Press. 1982. Pp. xii, 291. \$28.50.

Growing out of his PhD dissertation at the University of Wisconsin, Anthony N. Stranges's book relates the development of the electron theory of chemical bonding as it evolved over the first quarter of the twentieth century. This is essentially the intellectual history of a small group of men in Europe and the United States who struggled with the implications of J. J. Thomson's 1897 "discovery of the electron" for the theory of valence. There were immense difficulties in understanding precisely the nature of the atomic unions that form the molecules of matter with which we are most familiar.

At the turn of the century the chemists' atomic theory looked suspicious to many, if not most, physicists. Excitement over electromagnetism, electrodynamics, subatomic radiation, particles, and fields was so great that chemists studying the ultimate nature of matter were shaken by physicists studying the ultimate motions of matter. Although there were far more chemists than physicists in the world of the early 1900s, chemistry itself was in a highly descriptive and empirical state compared to the rational mechanics of physics. Stranges's work admirably traces the convoluted development of an adequate theory for the complex combination of atoms and molecules through the synthesis of a "shared electron pair bond" achieved by Gilbert Newton Lewis (1875–1946) in his studies at Berkeley from 1916 to 1923.

Although more than two hundred characters are indexed herein as major or minor contributors to "The New Theory of Valence," which is the title of Stranges's eighth and final chapter, only sixteen are illustrated with portraits, honoring roughly their chronological importance. J. J. Thomson heads the list, followed by Richard Abegg, Alfred Mayer, Johannes Stark, Lauder Jones, K. George Falk, Julius Stieglitz, Harry S. Fry, William C. Bray, J. U. Nef, Albert A. Noyes, Howard J. Lucas, Irving Langmuir, Gilbert N. Lewis, Wolfgang Pauli, and William A. Noyes. The list of honorees is curious not only for its order but also for its emphasis on German and American chemical theorists. French, British, and other national schools of thought as well

as physical theorists and experimental or industrial chemists, get short shrift. Scientific instrumentation and experimental designs are virtually ignored in this account, perhaps properly so, but even more curious is how this tale can be told without mention of J. W. Gibbs, Goudsmit and Uhlenbeck, P. A. Guye, Nernst's heat theorem. Jean Perrin, Schroedinger, or Einstein. Even the history of science, alas, is becoming superspecialized.

Beginning with Berzelius, Dalton, Davy, and others in a chapter entitled "The Nineteenth-Century Origin of the Electrochemical Bond and Its Failure in Organic Chemistry," Stranges proceeds to a set of three chapters wrapped around J. J. Thomson's electron and first atomic models, their reception, and electrostatic valence theory crossing the Atlantic. Chapter 5 deals with the long search for "electromers," as elusive as the luminiferous aether, the failure of which led to "The Decline of the Electrostatic or Polar Theory of Valence" (chap. 6). Having disposed of the inadequacies of unitary and electroatomic theories of valence, Stranges shows how a dualistic approach through the idea of an electron pair bond began to explain the behavior and properties of all combining atoms. Here is Stranges's strongest chapter, based on the G. N. Lewis papers at Berkeley. From his theory of the cubic atom first conceived in 1902 to the tetrahedral structure adapted in 1916, Lewis wrestled with his electron dot formulas and notions of the shared electron pair bond, as well as with the contradictions between his models of static atoms and magnetic electrons. Neils Bohr's dynamic atom, Langmuir's covalent bond, and Nevil Sidgwick's theory of inclusive orbits had resolved most of these conflicts by 1923. The introduction of quantum mechanics and Pauli's exclusion principle in 1925 placed the capstone on this monument to human understanding.

LOYD S. SWENSON, JR.
University of Houston,
University Park

MICHAEL BLISS. *The Discovery of Insulin*. Chicago: University of Chicago Press. 1982. Pp. 304. \$20.00.

The discovery of insulin bears many similarities to that of anesthesia. In each breakthrough the way had been well prepared, the time was ripe, four main characters played roles, bitter quarrels ensued over the assignment of credit, and subsequently a goodly number of other individuals claimed priority in the discovery. The essential difference is that insulin was a team effort whereas, though some collaboration existed between three of the participants, the discovery of anesthesia was an individual matter. In 1889 Oskar Minkowski and Joseph von

Mering observed that removal of a dog's pancreas caused diabetes. This finding touched off a flood of research on the relationship between the pancreas and diabetes, an incurable disease that brought young people a horrible and lingering death. Minkowski was the first to administer pancreatic extracts to diabetic animals, but the experiments were not successful. In the succeeding years several hundred other researchers experimented on both humans and animals with these extracts, but to no avail.

F. G. Banting was a young, insecure Canadian surgeon with a rather difficult personality. While trying unsuccessfully to establish himself in private practice, he chanced on an article on the islets of Langerhans, and decided that if he ligated the pancreatic duct, causing the pancreas to degenerate, he could then obtain the mysterious internal secretion from the islets. He approached J. J. R. Macleod, a professor at Toronto University, with his idea. After warning him that others had tried similar methods, Macleod gave him some dogs for experimental purposes, space in a laboratory, and assigned a young graduate student, Charles Best, to help him. As the work progressed, Macleod became more interested and gradually involved his staff members in the project, the most significant of whom was J. B. Collip, a professor in the biochemistry department. After many advances and setbacks, Collip finally managed to achieve a relatively purified form of insulin.

In the meantime Banting had become bitterly resentful of Macleod, accusing him of giving only reluctant help and of taking undue credit. When the Nobel Prize Committee named Banting and Macleod Nobel winners in 1923, Banting's first reaction, growing out of his animosity for Macleod, was to refuse the prize. Dissuaded from this, he announced that he was sharing his award with Best. Macleod in turn stated that he would share his award with Collip. After thoroughly examining every scrap of evidence and interviewing all living participants and witnesses, Michael Bliss, the author of this study, concludes that the prize committee made the correct decision, and that Best and Collip played important but lesser roles in the discovery.

This brief sketch cannot do justice to Bliss's narrative. The search for insulin was difficult and troubled by dead ends and personality clashes, yet he unravels the many threads, gives a clear exposition of a highly technical subject, and presents a well-balanced picture of the main figures. No historian's work is the final word on a subject, but I doubt that the main outlines of this event as delineated by Bliss will be changed.

JOHN DUFFY
University of Maryland,
College Park

JOHN W. CELL. *The Highest Stage of White Supremacy: The Origins of Segregation in South Africa and the American South*. New York: Cambridge University Press. 1982. Pp. xiv, 320. Cloth \$34.50, paper \$8.95.

The widespread neglect of research into comparative race relations seems a lament of the past. Comparative explorations of the American South and South Africa in particular abound. After George Fredrickson's widely acclaimed *White Supremacy*, Stanley Greenberg's *Race and State in Capitalist Development*, and Howard Lamar and Leonard Thompson's edited reassessment of *The Frontier in History*, John W. Cell's book is the fourth substantial treatment of the topic in three years. The initial skepticism about "what else is new?" soon gives way to the pleasure of nuances. This is a work of synthesis rather than original archival research. As a critical survey of diverse themes it succeeds remarkably in condensing the vast literature into its controversial highlights, despite some striking shortcomings.

Cell's main thesis is simple. Unlike Fredrickson's comprehensive treatment of white supremacy, Cell's singular focus on segregation as an ideology makes him stress discontinuity. He believes that in both societies "segregation as a system was essentially new," rather than the mere outflow from slavery and the frontier past. Cell emphasizes the political choice of class actors, who invented segregation among various other possible policy options of privilege maintenance. Why segregation? Cell stresses three factors. First, under paternalistic feudalism the proximity and ready availability of cheap labor remained crucial for agricultural production, while in the more competitive industrial setting color bars and policies of exclusion preserved privilege. Second, the ideology of segregation subjected the dominated to a "self-enforcing" system of inward self-deprecation, "a complex fabric of structural ambiguity" (p. 18) under the guise of constitutionality. But, third, and even more important, Cell argues, segregationist ideology provided the heterogeneous ruling group with a successful, convenient formula to which all could agree "while continuing to conflict." When a common economy drew people together, separate development "was to soften class and ethnic antagonisms among Whites, subordinating internal conflicts to the unifying conception of race" (p. 234).

Race as the least common denominator for a divided ruling group indeed makes sense, but it hardly represents the break with the past that the author makes it out to be. Indeed, in this view race is not the irrational anachronism of the liberal school. Racial ideology, however, also does not coincide with the epiphenomenal role it has been assigned by neo-Marxist revisionists with whom the author rath-

er uncritically and explicitly aligns himself (p. 58). Most adherents of the Marxist perspective, incidentally, would cringe at a statement that "the driving force, the ultimate cause, behind segregation was white racism" (p. 3). If race, like class consciousness, embodies historic specificity, they can hardly be considered ultimate causes. It is also not enough to stress that race and class "intersect" (p. 17) in complex ways, but how, when, and why?

Nevertheless, this study represents a challenging, richly textured argument. Especially when the author attempts to assess the impact of segregation on the militancy and acquiescence of its victims, he opens many uncharted horizons with original pointers. Its strengths and weaknesses have yet to be explored, Elkins, Vann Woodward, Genovese, Barrington Moore, or E. P. Thompson notwithstanding.

HERIBERT ADAM
Simon Fraser University

ANCIENT

JOHN BOARDMAN and N. G. L. HAMMOND, editors. *The Cambridge Ancient History*. Volume 3, part 3, *The Expansion of the Greek World, Eighth to Sixth Centuries B.C.* 2d ed. New York: Cambridge University Press. 1982. Pp. xvi, 530. \$49.50.

Fifty-eight years have passed since the appearance of volume 3 of the *Cambridge Ancient History*. So greatly has our knowledge of the ancient world increased in that span, that volume 3 of this new edition is to be published in three parts, each a separate volume in its own right. The subject of part 3, under review here, is expressed by the subtitle: "The Expansion of the Greek World, Eighth to Sixth Centuries B.C." There are seventeen chapters by twelve authors: two chapters on the Greeks in the Near East and Egypt respectively by T. F. R. G. Braun; Cyprus by V. Karageorghis; the Cypriot syllabary by T. B. Mitford and O. Masson; colonial expansion and the western Greeks by A. J. Graham; the eastern Greeks by J. M. Cook; Crete by John Boardman; Cretan laws and society by R. F. Willetts; one chapter on Euboea and the islands and another on central Greece and Thessaly by W. G. G. Forrest; a chapter on Illyria, Epirus, and Macedonia; and one on the Peloponnese by N. G. L. Hammond. Two chapters by A. Andrewes trace the development of Athens from the unification of Attica through the Pisistratid tyranny, while the final two chapters, economic and social conditions by C. G. Starr and the material culture of Archaic Greece by John Boardman, provide the only overview of the period as a whole. There are eighteen maps well

placed throughout the text, sixty-three line drawings, a chronological chart, a bibliography, though a very select one, of forty-one pages, and an index. A volume of plates to accompany the three parts of volume 3 is promised.

The period from the eighth to the sixth centuries is a difficult one to come to grips with. The basic problem is one of sources, for there is not much in the way of contemporary literary or epigraphical evidence. Herodotus supplies much useful information and Thucydides is occasionally helpful, but much of our knowledge about the period comes from Greek writers who lived long after the sixth century came to an end. Neither epigraphical nor literary evidence has increased greatly over the past fifty-eight years, but archaeological excavation has provided much additional information. Although the archaeological evidence had little to contribute to the subjects immediately at hand in the first-rate chapters on the Cypriot syllabary and Cretan laws and society, it is used with varying degrees of success throughout the remainder of the volume. Braun has nicely combined the archaeological and literary material, both Greek and non-Greek, in his treatment of the Greeks in the Near East and Egypt. It is put to good use in the chapters on the eastern Greeks, Crete, and the material culture of Archaic Greece, and in a more general but equally effective way in the discussion of economic and social conditions. The chapter on Illyria, Epirus, and Macedonia, however, will be more useful as a summary of the archaeological evidence than a history of the area.

Yet it is striking to observe just how little archaeology has contributed to our understanding and interpretation of political events, and the extent to which we are still largely dependent on the literary tradition. Although the literary sources are better for Athens than for any other Greek polis, even here they leave much to be desired and must be used with caution. A. Andrewes employs them judiciously in his fine treatment of Athenian development, but the careful reader of these pages will be struck by how much we do not know even about Athenian history in the years just prior to 500 B.C. For areas outside Athens, the literary evidence is, generally speaking, more meager, more confused and contradictory, later in date, and must be used with even greater caution. Yet this is an element frequently lacking. Numerous examples might be cited, but a good case in point is N. G. L. Hammond's discussion of chronology (pp. 321-25), where it is confidently and unequivocally asserted that Olympic records provide us with a secure chronology extending back into the eighth century B.C. The matter is not so neatly cut and dried as presented here, and the general reader will be much better served by H. T. Wade-Gery's more prudent

discussion of chronological problems in the original edition (pp. 761–65).

It is not easy to generalize about a work that boasts two editors and twelve authors. There are the expected and unavoidable problems of repetition, diametrically opposed interpretations of the same evidence and unevenness of style. Some chapters are lucidly written with the general reader in mind, others seem intended for a more specialized audience. As a whole, the volume tends to focus rather narrowly on political history and archaeological data, while literature, art, and religion, for example, are treated incidentally if at all. The result is a somewhat narrow picture of the overall expansion that characterized two of the most exciting centuries in Greek history. Individual chapters run the full range from superb to poor, however, and each must be judged on its own merits.

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CHARLES THOMAS. *Christianity in Roman Britain to A.D. 500*. Berkeley and Los Angeles: University of California Press. 1981. Pp. 408. \$35.00.

This is a welcome addition to the flood of new books on Roman Britain. Its very title is a challenge: how Roman was Britain in the fifth century? Chapter 10 provides some stimulating new material in answer. Compared with other Latin provinces, the evidence for British Christianity is meager: no church councils or great clerics' careers in their native land described, to match Elvira, Saragossa, and Toledo, or Priscillian and Martin. If only Pelagius had formulated his heresy in Britain and had been called to book there or at Trier instead of in Rome, Carthage, and Jerusalem. Even the channels by which his teachings penetrated Britain remain totally mysterious.

Charles Thomas grapples heroically with the historical evidence at the outset, but it is the later chapters, where he deploys all his archaeological resources, that will be found most valuable: two substantial chapters (4 and 5, pp. 96–142) deal with "The Material Evidence," followed by discussions of church buildings, baptistries, and burials. Chapter 10, on "Fifth Century Britain and the British Church," (pp. 240–74) is notable for its distribution maps of place-name evidence. There follow chapters on Ninian and Christianity in southern Scotland, three on Patrick, and a conclusion.

On Patrick, Thomas is bold, locating his home near Carlisle (the *Banna* on Hadrian's Wall) and preferring the later chronology (from 415 to 490–5). He does not try to justify his preference for these dates against what has become the standard view, but it emerges that he attaches some importance to

Dolley's work on hoards (*Proc. R. Irish Acad.* [1976]: 181 and following) and to arguments for a later dating of Coroticus, addressee of Patrick's *Epistola*. It might have been helpful to have set this out at the beginning of his discussion.

Some points of detail require comment. He appears to believe that Magnus Maximus had a British wife called Helena and a son called Constantine (p. 41). "Tetrarchy" is mistakenly applied to Julian's time as Caesar (p. 103). Albeit cautiously, he cites the "Wroxeter Letter" found at Bath in 1880 and published by E. W. B. Nicholson (p. 126 n.). This must have been a leaden curse tablet, of which a new batch was found in 1979. Whether there was really reference to a "dog of Arius" (*canem Arii*)—and if so whether this has anything to do with the heresiarch, must remain doubtful. But it is to be hoped that the tablet still exists and can be reread, for one of the new tablets proves to curse anyone "whether pagan or Christian" (*seu gentilibis seu Ch(ri)stianus*) (*Britannia*, 13 [1982]: 406). A pity that this fascinating item was not available to Thomas.

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MEDIEVAL

BENEDICTA WARD. *Miracles and the Medieval Mind: Theory, Record, and Event, 1000–1215*. (The Middle Ages.) Philadelphia: University of Pennsylvania Press. 1982. Pp. x, 320. \$25.00.

With this fine revision of her Oxford thesis, Benedicta Ward continues her excellent contributions to monastic and medieval scholarship, most widely known of which is her introduction to and translation of *The Prayers and Meditations of St. Anselm* (1973). With *Miracles* the author hammers more nails into the coffin of the notion, which should long ago have been put to rest, that the Middle Ages constituted a monolithic culture. The mind was as complex and variable in the Middle Ages as in any other era. Not only did the outlook on miracles recorded at shrines differ from reports of miracles appearing in twelfth-century hagiography, but various circumstances altered interpretations even within the same genres of records. This book makes one wary of generalizations about miracles. Moreover, because it deals with so pervasive a theme in medieval culture, this study is an entree into both the mentality of the unsophisticated and into the mind of authors capable of a quite subtle understanding of miracles. As Ward herself points out, her investigation may even provide an improved approach to the study of the biblical miracles, a study "in which demythologizing long ago reached a dead end" (p. 215).

Ward's book is useful both for the specialist in the religious history of these centuries and for anyone who is interested in gaining insight into the distinctive attitudes that shaped medieval consciousness. Despite Ward's admission that her conclusions must be tentative and preliminary in the face of the massive amounts of materials, this pioneering study lays a firm footing for future research and will regularly be consulted for a long time. *Miracles* is clearly and expeditiously written. The author does not endlessly follow every lead she mentions. Of course, there are instances where one wishes that she had said more about one's pet interests. For the satisfaction of many readers, I think she could have summarized briefly Thomas Aquinas's doctrine on miracles even though Thomas taught about a half century after the end of the period she is studying. Nonetheless, Thomas developed his doctrine, as the author acknowledges, out of the tradition that had been inaugurated by Augustine. Yet, in fairness to Ward, her chief concern is with the analysis of actual records of miracles and the meaning that thus emerges rather than with the evolution of more theoretical views of miracles.

As I read *Miracles*, I found myself making extensive notes because it is a theme that impinges on so many facets of medieval life. To name but a few: hagiography, the process of canonization, Marian devotion, and the building of shrines and cathedrals. For this reason the index of persons and places could have profitably included subjects. The author could also have been more precise about dates at various points of her narrative and also about the relative mix of manuscript and printed sources. These are, however, slight drawbacks in a first-class and lasting contribution to the understanding of a complex and neglected theme in medieval culture.

KEITH J. EGAN
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ROBERT BARTLETT. *Gerald of Wales, 1146–1223*. (Oxford Historical Monographs.) Oxford: Clarendon Press of Oxford University Press. 1982. Pp. 246. \$46.00.

A scion of a family of marcher lords of mixed Welsh and Norman descent, a member of an ecclesiastical dynasty and would-be bishop and archbishop of St. Davids, the intellectual product of the schools of Paris, a clerk in the service of Henry II of England, and the author of treatises on Wales and Ireland as well as on a multitude of other subjects, Gerald of Wales is one of those twelfth-century figures who are of far more than local significance.

Although Robert Bartlett's first section deals with the themes of politics and nationality, he does not

attempt a detailed treatment of Gerald's public career, in particular his ecclesiastical ambitions, which have been fully discussed elsewhere. This allows him in his other two sections to provide a thorough examination of other aspects of Gerald's work, his studies in ethnography and of the natural and supernatural.

Bartlett's discussion of Gerald's ethnographic achievement passes from the interesting point that he distanced himself from both the English and the Welsh by using the term *Kambrenses* rather than the more usual *Wallenses* and *Britones*, employed by the English and the Welsh respectively when describing the inhabitants of Wales (p. 185), to a wider treatment of Gerald's attitudes toward the Welsh and their neighbors, the Irish. Although Gerald could not, as an ecclesiastic, avoid condemning what he saw as the lax sexual and marriage customs of the Welsh, he was nonetheless proud of his Welsh ancestry: his advice on the best means of conquering the Welsh was accordingly balanced by advice on how they should resist conquest. In some respects Gerald's attitude toward the Irish was similar, except that his condemnation of their barbaric practices was not counterbalanced by any sympathy, largely because of his close family relationship with many of the Norman invaders from west Wales who were then conquering Irish territory. Bartlett gives added significance to his treatment of Gerald's views on the two Celtic peoples by comparing them with medieval attitudes toward other fringe societies, notably those of the Baltic and Slav lands, both of which were, like Wales and Ireland, subject to penetration and conquest by men from the more settled areas of western Europe. The difficulties that the "civilized" world had in comprehending that of the barbarians are also illustrated by reference to the thirteenth-century accounts of the Mongols given by Carpini and Rubruck.

Gerald of Wales emerges as an enthusiastic but not necessarily highly original observer in his studies of both ethnography and natural sciences. As Bartlett remarks, he was better at observation than at theory and would have made "a good zoologist but a bad physicist" (p. 153). Bartlett has produced a well-written and stimulating account of Gerald's opinions and has added to its value by a wide-ranging comparison between him and his contemporaries as well as by an extensive acquaintance with modern scholarship. His book is to be strongly recommended.

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DESMOND SEWARD. *The Hundred Years War: The English in France, 1337–1453*. Reprint. New York: Atheneum. 1982. Pp. 296. \$8.95.

Desmond Seward has us all in his debt. He has given us popular readable accounts of a great variety of subjects on which good modern books at that level are simply not available. He does his best to be up-to-date without sacrificing his main goal, to give a lively narrative, as uncluttered as possible by argument and apparatus. Narrative history has not been very highly regarded in recent years. Few scholars can write it or bring themselves to write it. So it is good that Seward and his kindred rescue the genre from the hands of mere journalists.

Readers of the *American Historical Review*, however, will wish for a more scholarly appraisal. In this regard, Seward has indeed ignored a rather considerable amount of academic research and debate. In trying to avoid analysis and qualifications, he resorts sometimes to "perhaps," or to very general observations such as "some historians differ." An example or two will serve to convey the level at which he is writing. On the subject of gunpowder and cannon, he describes vividly how and when it was used without posing the problem, recently raised by M. Vale in a contribution to C. T. Allmand's *War, Literature and Society* (1978), of whether its use at Harfleur by Henry V can be taken as marking a revolutionary change in warfare. Yet one feels that he has looked at the writing on the subject with a keen eye. With regard to naval warfare, he must score well for his use of C. F. Richmond (writing for K. Fowler's *The Hundred Years' War* [1971]), but he could have improved his story with proper use of Timothy Runyan's publications.

His account of armies and their recruitment unduly emphasizes the role of commissions of array (following my own error, I fear), but, more seriously, he has not absorbed the message of A. E. Prince on the subject of numbers. He does balk at the worst exaggerations but so did many earlier scholars. Numbers for the 1340 and 1341 campaigns, for example, remain inflated. Reader, beware!

It is to be expected that the handling of diplomatic aspects of warfare is less skillful in such a book than is that of battle and siege. Although listed in his bibliography, for example, J. J. N. Palmer's account of Richard II's peace negotiations is not used with much care and is even rather flagrantly ignored at times (for example on the proposals regarding the papal schism at the time of Richard's wedding to Isabella of France).

The bibliography is useful, if highly selective. The main weakness lies in the listing of sources, where a concentration on translated versions would have been preferable given his likely readership. It is useful to be reminded, however, how poor is our record in translating medieval French historical material.

MICHAEL R. POWICKE
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RAYMUND KOTTJE and HARALD ZIMMERMANN, editors. *Hrabanus Maurus: Lehrer, Abt und Bischof*. (Symposion der Akademie der Wissenschaften und der Literatur, 1980; Abhandlungen der Geistes- und Sozialwissenschaften Klasse, number 4.) Wiesbaden: Franz Steiner. 1982. Pp. xii, 208. DM 63.

To an American readership, with its residual orientation toward Anglo-Saxon roots and perspectives, the Carolingian author and educator Hrabanus Maurus is probably best introduced as a disciple of Alcuin and a Beda Venerabilis in Teutonic sheep's clothing. Curiously, this severe figure at the dawn of a distinct German cultural tradition, who deeply influenced German learning and letters, has often been denigrated as a ponderous schoolmaster and, in the unfortunate phrase of E. R. Curtius, as "a tiresome compiler." The work of a number of medievalists over the last twenty-five years, almost exclusively in Germany, has done much to define more accurately Hraban's goals and methods and consequently to assess more fairly his historical achievement.

In the present selection of papers, given at a commemorative symposium in 1980, the introductory and concluding essays, by Franz Brunhölzl and Josef Fleckenstein respectively, summarize this revised picture of Hraban, as an important commentator on the Scriptures who wanted to offer not new interpretations but abundant information; an encyclopedist in the manner of Isidore of Seville who adopted an original principle of organization and pursued his own peculiar objective, to arrive at the underlying meaning of things; a poet of considerable technical virtuosity; a teaching abbot who made his monastery of Fulda a center for copying ancient and ecclesiastical texts and educated several important authors; a scholar whose life's work helped assure the survival of Carolingian learning when the political and administrative structures of the empire crumbled; and one of the essential links with the classical tradition for both scholastics and humanists.

The remaining essays in this volume contribute at least in part to the foundations for such an assessment. Eckhard Freise and Franz Staab review the basic chronological data that are still in question: Hraban's year of birth (the evidence for 780, it is shown here, does not exist) and of his joining the community in Fulda, apparently 788. On the other hand, Karl Schmid is able to add to an understanding of Hraban's spirituality from an examination of the Fulda necrologies, whose edition and study he has initiated. And John M. McCulloh's analysis of Hraban's martyrology gives one a better grasp of Hraban's general scholarly methodology. Herrad Spilling makes clear the impulse Hraban provided for heightened activity in the Fulda scriptorium. The other contributors show us different aspects of

Hraban's work as well as the limitations of his range. According to Wolfgang Haubrichs, Otfried of Weissenburg, author of an influential gospel harmony, took his cue from Hraban in biblical exegesis but not in choosing to translate the gospels into the vernacular. Gaugolf Schrimpf demonstrates convincingly (though others have made the point before) that Hraban, who merits a footnote in the history of Christian doctrine because of his role in the controversy over predestination, misinterpreted Gottschalk's rather enlightened and socially useful position. Raymund Kottje examines Hraban's interest in problems and sources of the law and finds it inspired by pastoral concerns; though not a mere compiler, Hraban was not a great canonist. However, his own legislative activity as archbishop of Mainz, during the last eight years of his life, discussed here by Wilfried Hartmann, reveals common sense as well as a determined will to reform his church.

These essays reflect their origin as papers presented by working specialists to others of the same kind. They vary in degree of significance for an understanding of the Carolingian scholar but collectively provide us with a clear notion of where scholarship on Hraban stands now and what problems of his work and influence remain to be explored.

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PAULINE MOFFITT WATTS. *Nicolaus Cusanus: A Fifteenth-Century Vision of Man*. (Studies in the History of Christian Thought, number 30.) Leiden: E. J. Brill. 1982. Pp. 248, f 78.

Until relatively recently Nicholas Cusanus has been the Melchizedek of the history of Christian thought—without parentage and with only a few contested paternities. Giordano Bruno seems to have inherited the same blood type, but fatherhood of Kant's noumenal and phenomenal world seems a long shot, with many more likely sires.

With Pauline Moffitt Watts's *Nicolaus Cusanus: A Fifteenth-Century Vision of Man* we have an attempt to see Cusanus resolutely in the context of the spiritual and intellectual problems of late medieval and Renaissance culture. More specifically, Watts attempts to trace his conception of human nature and its capacities to his assimilation and transformation of the four great traditions of his age: scholasticism, humanism, Platonism, and mysticism.

It is a task that cried out to be done, and it is done here with exemplary success. As examples of connections between Cusanus and these traditions the following, in particular, should be noted. Cusanus's principle that "there is no proportion between the finite and the infinite" paralleled the nominalist

dialectics of the *potentia Dei absoluta* and the *potentia Dei ordinata*. But Cusanus went beyond the scholastics in setting up more radical grounds for the absolute freedom and transcendence of God.

The study sparkles in its delineation of this German Hercules's connections with Italian humanists: Toscanelli, Cesarini, Bracciolini, Traversari, Lorenzo Valla, and others. Cusanus's significant humanist associations paid rich dividends in bringing to his learning and library the new fifteenth-century translations of Plato and the Platonic tradition. He appropriated the Platonic dialectic of unity and plurality while rejecting the existence of intermediaries or agents between God and the creation. Here Watts gives due attention to Cusanus's twelfth-century Platonic sources, in particular Thierry of Chartres. The transmutation of mysticism is examined in conjunction with Cusanus's application of images of light and sight in two later writings, the *De visione Dei* and the *De Beryllo*.

The study represents a careful analysis of each of Nicholas Cusanus's major works, beginning with the *De docta ignorantia* (1440) and ending with the *De apice theoriae*, written a few months before his death in 1464.

Cusanus's vision of man, the universe, and God has most commonly been seen in terms of a fundamental breakthrough or reorientation, attested by the author himself, achieved in the *De docta ignorantia*. Through her *Gesamtwerk* approach, tracing the major themes and developments in five main chapters, Watts is able to show not only the formidable disjunction of man from creator and natural world but also the transcending principle Cusanus evolved from the four great traditions he transmuted: he found the ultimate creative force in the human mind itself, created in the image and likeness of the divine mind. So Watts traces an affirmative anthropomorphic principle, anchored in an exploration of Genesis 1: 26, which counteracted the impersonal disjunctive metaphysic that dominated Cusanus's earlier thinking. Man in his unique cosmic freedom created a world of conjectures, a world of objects, and a world of play that proved his creation in God's image and likeness. Here the author is even able to trace a convincing link to Pico's affirmation of freedom and dignity.

No *dulce loquentem* this, but a happy conjunction with Charles Trinkaus's *In Our Image and Likeness*, with the humanity and divinity in Italian humanist thought.

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PETER PARTNER. *The Murdered Magicians: The Templars and Their Myth*. New York: Oxford University Press. 1982. Pp. xxi, 209. \$29.50.

Peter Partner's best-known books hitherto have dealt with Rome and the papacy in central Italy. Now he has aimed his pen at the downfall of the military order of the Knights Templar, in which the papacy played merely a supporting role, and at the postmedieval mythology that grew up around the Templars, a development in which the papacy found no role at all.

The first part of *The Murdered Magicians* deals with the history of the suppression of the Templar Order and analyzes the complex historiography that has grown up around it. The discussion is intelligent and well informed, but not startlingly novel. Partner has no new thesis to expound, no innovative explanation to propose for the Templars' much-controverted downfall. By and large he subscribes to Heinrich Finke's conclusions on the matter, which is to say that he accepts the more-or-less conventional view that has dominated Templar historiography since the beginning of this century. Partner is properly skeptical of the more extreme efforts to exculpate the Templars totally from the charges against them; he likewise rejects the simplistic interpretation that Philip the Fair set out to get the Templars solely because he wanted to grab their assets. Although Partner's treatment of the suppression of the Templars proposes no new interpretation and furnishes no new evidence, his brief account is perceptive and gracefully written. This makes it a useful, though scarcely earth-shattering, addition to the English literature on the topic.

The second part of *The Murdered Magicians* ventures into more exotic territory—the legends, myths, and ghost stories that grew up about the Templars after their order's demise. The Templars may have been, by and large, a rather stodgy, unimaginative, self-centered lot while their order existed; Partner thinks, on the whole, that they were, and he is probably right. After they had ceased to exist, the imagination, and sometimes the sheer lunacy, of later generations supplied the Templars with a romantic luster that reality notably lacked. Henry Cornelius Agrippa of Nettesheim turned the Templars into magicians in his *De occulta philosophia*, first published in 1531, and from that point on there was no stopping them. The subsequent history of the Templars' reincarnation in various guises, most notably as a branch of the Masonic movement, is a fascinating study in the history of fantasy and fraud. Partner tells this tale with relish. He is considerably indebted, to be sure, to the researches of Le Forestier, Chevalier, and other earlier explorers of these labyrinths. But Partner's account has its own value as a worthwhile specimen of lively and intelligent intellectual history.

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MODERN EUROPE

P. J. MARSHALL and GLYNDWR WILLIAMS. *The Great Map of Mankind: Perceptions of New Worlds in the Age of Enlightenment*. Cambridge: Harvard University Press. 1982. Pp. 314. \$22.50.

One of the great themes of modern European intellectual history is the impact on the Western mind of new contacts and a new knowledge of unfamiliar cultures, especially in the seventeenth and eighteenth centuries. As Europeans discovered the non-Western world a variety of responses proved possible. New knowledge could result in either greater understanding or greater incomprehension; it could encourage a profound relativism or it could reassure Europeans in the superiority of their own values. In fact, it seems to have done something of all these things and simultaneously, although P. J. Marshall and Glyndwr Williams argue that by 1800, despite much new information, it was largely incomprehension and superiority that predominated in England. They see the net result of increased contact in the development of a view that furnished a preparation and justification for nineteenth-century imperialism.

To be sure, the authors, both of whom have written previously on imperial themes, are no simple reductionists. They allow that scholars and men of affairs have often had different motives and interests and that the development of "assumed" knowledge and the growth of imperial power were not simply interconnected and coordinate. They have tried to describe an interaction that extended over two centuries. In the first half of the book they tell of the English encounter with the higher civilizations of Asia, from the Near East to India, China, and Japan; in the second, they show how England reacted to new knowledge of the simpler peoples of America, Africa, and the Pacific Ocean. In each part, there is some sense of a development of ideas from an utter to a relative incomprehension, from awe and admiration to a growing disenchantment and feelings of superiority. Exceptions are admitted only to prove the rule.

The value of the attempt lies in its scope, which is wide, and its documentation, which is up-to-date. Even so, concentration on England is awkward, since so much of the information and attitudes of the English were shaped by the work of other Europeans; and the documentation, which takes account of much recent scholarship, is concentrated largely on well-known books. Much of the information and many of the quotations seem to derive from secondary accounts. The authors know and use some general works of intellectual history, like Lovejoy and Manuel, but this is not their forte. Like many others who have turned back to the eigh-

teenth century, they are intent on discovering the roots of modernity. As modern liberal relativists, however, they have necessarily been disappointed in what they found, though they seem to have found pretty much what they were looking for. The English, they tell us repeatedly, largely made and unmade the rest of the world to fit their own preoccupations, rather than altering their perceptions in the light of new knowledge. Their tale is a story of prejudice and distaste, of ethnocentricity and self-interest. It does not seem to have occurred to the authors that their own views and values might have had an equally distorting effect on their own perceptions of a foreign (that is, eighteenth-century) culture.

Still, this is a useful book of its kind. The authors have gathered much information and quotation and thereby furnished a useful introduction to an important subject. (Some maps and pictures would have helped.) Perhaps they will give us a second volume and carry the tale into modern times, where they will have the advantage of a more congenial, or at least a more familiar, ground.

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ULRICH IM HOF. *Das gesellige Jahrhundert: Gesellschaft und Gesellschaften im Zeitalter der Aufklärung*. Munich: C. H. Beck. 1982. Pp. 263. DM 58.

Synthesizing a number of recent works, this book examines the multifarious societies and associations that proliferated as part of the Enlightenment. Ulrich Im Hof excludes religious associations and guilds as part of an earlier world and *Weltanschauung*, and salons and clubs as similar in spirit to his societies but lacking formal organization. The societies in question aimed at improvement that could not be accomplished by existing institutions: they were voluntary associations that admitted members without regard to estate or confession.

Im Hof classifies the societies into five types: scientific and learned academies, literary and reading societies, public-spirited (*gemeinnützige*) associations (for commerce, industry, poor relief, education, and health), economic-agricultural societies, and patriotic-political societies. Freemasons and Illuminati had structure and interest similar to many of these societies, and often had overlapping membership. The author provides brief histories of about twenty-five of the hundreds of societies and generalizes about "national variants" in the movement. Germany, Switzerland, France, Britain, and Italy, for which considerable literature exists, are best covered. Iberia, the Netherlands, Scandinavia, and Eastern Europe get more cursory treatment.

Although admitting that the academies of Renais-

sance Italy, transplanted all over Europe in the seventeenth century, were precursors of his societies, Im Hof sees the latter as essentially novel, not only in numbers but also in extension beyond purely scientific and scholarly aims and in their abandonment of "baroque" ceremonialism and rigidity in favor of a more egalitarian sociability. Spreading from Britain, the movement responded to the growth of knowledge and the desire to increase and diffuse it for improvement. From the beginning the societies wanted to join theory and practice, but their direct practical accomplishments were limited. Most societies hardly survived beyond the enthusiasm of their founders. The public-spirited and agricultural societies often grappled with problems unsolvable without great political, economic, and social changes. But to survive, they had to remain apolitical and avoid facing deeper, unsettling issues. This contradiction (which Im Hof insufficiently emphasizes) explains the high-minded sterility of much associational activity. Aside from scientific and learned societies, whose work was not directly practical, the most successful groups flourished in the free atmosphere of Britain, or worked closely with the regimes of the German and Swiss city-states. The suppression of some societies in the 1780s shows the waning of the Enlightenment and prefigures the counterrevolution of the 1790s.

The societies were small republics of equals, representing elites of diverse origins. Vehicles of a new "social consciousness" (p. 185), they were an early form of Jürgen Habermas's *bürgerliche Öffentlichkeit*, breaking down socially and ideologically the separate publics of court, town, church, university, and government on the common basis of *Besitz und Bildung*, in the name of a new knowledge, reform, refinement, and public-spiritedness.

The first hundred pages of Im Hof's book are a cultural history of the eighteenth century pitched at the general reader and only loosely related to the remainder, which treats the societies and will interest all social and intellectual historians of the eighteenth century.

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ALAN EDELSTEIN. *An Unacknowledged Harmony: Philo-Semitism and the Survival of European Jewry*. (Contributions in Ethnic Studies, number 4.) Westport, Conn.: Greenwood. 1982. Pp. xii, 235. \$29.95.

In principle there may be much to be said for standing a historiographical convention on its head, notably when, as in the case of the book under review, the convention is exceedingly well established. The convention is that the long and often excruciatingly painful history of the Jews of Europe

was the product first and foremost (which is not to say exclusively) of the active hostility that the majoritarian inhabitants of Europe bore them. No doubt the Jews had a history of their own. In certain important respects they were subjects, rather than objects, after all. They evolved socially and intellectually; they had (their own) learning, theology, literature, and art; they were organized in communities; and, toward the end, they had politics as well. But, so we have learned to think, throughout the long period extending from the early Christian era to the final catastrophe of 1939–45, their lives and thoughts were colored, and to a very considerable degree determined, by the fragility of their status, the absence of commonly accepted rights, an enormous social hostility, and continual physical danger.

It is, admittedly, a dark picture and, if one dwells on it, depressing; altogether too dark and too depressing, Alan Edelstein seems to wish to tell us. For there is, he believes, a brighter side (*philo-*, as opposed to *antisemitism*) and the bright side is historically important: so important, indeed, that in his view, the "survival of European Jewry as a salient group and European Jews as individuals is due in a large degree to philo-Semites" (pp. 3–4).

Now, it may be asked, how and in what sense one can speak of the *survival* of European Jewry at all? And further, what might be meant here by "salient"? It may be that, in the author's mind, it is the undoubted fact that at no point in their long history were *all* Jews ever wholly and successfully ground into the dust that seems to require some sort of explanation. In that case it is as if what we must labor to account for is the fact that a fraction of European Jewry survived after all. But then surely the picture is darker still, and, as one considers its possible implications for the quality of European society and culture as a whole, vastly more depressing.

A greater weakness in the author's argument is that it is bound to rely on manifestations of "philosemitism" (such as they were) in Western and Central Europe only. Eastern Europe, where the bulk of Jewry was actually located in the modern era, is dismissed in a paragraph or two—rightly, so far as the book's thesis goes, for there was precious little "philosemitism" *there*. The history of Jewry, at least since Renaissance times, is, before all else, the history of its East European members; and of them it can certainly not be said, in the terms of one of the author's more astonishing dicta, that "Christian philo-Semitism" held them together "through confirming pride and self respect" (p. 201). Thus there remains the question—admittedly a large and difficult question—of the salience of events on the periphery of the main concentrations of Jews for the evolution of the Jewish people as a whole. But even of the relatively small communities of Jews in

Western and Central Europe this is a doubtful proposition, to put it very mildly. One may suppose that their extraordinary persistence in the face of extraordinarily persistent external pressure and hostility had more to do with their inner resources than with occasional manifestations of interest and concern from outside.

A word on the author's sources. That these are entirely secondary is curious, but not, perhaps, decisive. For one may take his point that he was engaged in an analysis of other men's histories, not in the writing of an independent account of his own. What may be more significant and what, one suspects, may provide a clue to one of the reasons for the failure of this ambitious but excessively superficial study, is the fact that such literature as Edelstein appears to have relied on is in the English language alone. There is no trace of the literature—admittedly vast in quantity and often obscure in content—in Hebrew and Yiddish; neither is there a hint of what exists in German, French, Italian, or Russian. Can one proceed with this subject in so restricted a way? Hardly, one feels. One needs a fuller range of sources not merely for specifics of fact and analysis, but for a sense of the real, underlying complexity of the Jewish scene (and therefore of Jewish historiography), notably in the times of greatest trial.

DAVID VITAL

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GEOFFREY BEST. *War and Society in Revolutionary Europe, 1770–1870*. New York: St. Martin's Press. 1982. Pp. vi, 336. \$27.50.

During the last generation the nature of military history has been revolutionized. The older preoccupation with battles and campaigns has been replaced by a more rounded emphasis on the economic, social, ideological, psychological, and technological dimensions of warfare. One banner for this advance has been that of "war and society." This approach has now produced the very promising "Fontana History of War and European Society" series, edited by Geoffrey Best. Best is also the author of the inaugural volume, which gets the series off to an admirable start.

Best's survey examines the ways in which the military world of the *ancien régime* was transformed by the wars of 1792–1815 and draws freely on the abundant recent monographic literature to provide a lucid and compact introduction that will be particularly useful for students. The central theme is the dramatic transformation of warfare and of the position of military forces within Europe produced by the French Revolution and by Napoleon. As would be expected from the author of the splendid and pioneering *Humanity in Warfare* (1980), there

are some fascinating pages on the impact of these developments on the European psyche. The most important legacy of this conflict, Best believes, was "its popularization of professional military activity for its own sake, its savouring of war as such" (p. 198). The most novel chapters are those on the years from 1789 to 1815, in which newer approaches to military history are effectively utilized. Thereafter the author provides a fairly conventional survey of the role of armies in particular states: here, as elsewhere, he is largely at the mercy of the existing literature, which effectively determines the contours of his survey.

Certain reservations should be made. Best occasionally seems to accept contemporary strictures about the poor quality of the rank and file at face value. His recurring emphasis on the compulsion usually needed to keep men in the ranks is overdrawn and minimizes the very real attractions of military service, not merely as a source of adventure and, perhaps, advancement, but as a better life than was to be had *outside* the armies. His approach also raises one general problem. Best is rightly critical of the old-style military history (p. 7), but his own study does not altogether escape the danger of *Hamlet* without the prince. The transformation that he is describing was primarily a response to the new scale and nature of warfare, but this is taken largely for granted. Perhaps the pendulum of historical fashion has swung too far and we now need to incorporate the more traditional concern with battles, tactics, and strategy with the newer perspectives so admirably employed in this study.

Best has distilled the unmanageable bulk of recent writing into a single volume that deserves a wide readership. It provides both an introduction to the decisive impact of the generation of warfare unleashed by the French Revolution and a distinct and, in places, novel perspective on a familiar period of European history.

H. M. SCOTT
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CLIVE TREBILCOCK. *The Industrialization of the Continental Powers, 1780–1914*. New York: Longman. 1981. Pp. xvi, 495. Cloth \$33.00, paper \$14.95.

Can economic history, uneasily resigned as it is to "testing" models of nineteenth-century industrialization (models devised so long ago their cogs and wheels are alarmingly creaky), still pretend to provide guides as to why some nations develop and others falter? Can a reasonably sized book encompass enough findings by indefatigable researchers over the past forty years in the economic history of Germany, France, Russia, Italy, Austria-Hungary, and Spain so that a reader can feel he is dealing with

the state of the art? Can a teacher of undergraduate European economic history courses find both conceptualizing and pedagogic happiness in this volume? Clive Trebilcock believes the answer to all of the above is yes, and I believe that many teachers will agree with him—but will find their work cut out for them in deriving maximum class benefit from this text.

Indeed, this is an admirably crafted book—except for the maps, dull grey blobs that repel more than they invite study. Trebilcock begins with a short but by no means oversimplified presentation of the development models of Rostow and Gerschenkron, including a discussion of their strengths and weaknesses. He shows Gerschenkron's approach from relative backwardness is superior but needs supplementing by considerations of entrepreneurial quality, national economic policy, and "anti-national" (international and regional) affairs. The central chapters present the Continental powers (not Britain, and not the smaller nations) in order of their success in overcoming backwardness and obstacles to growth. Chapter 25 on Germany, is a first-rate job, a great improvement. I believe, over Borchardt's essay in the "Fontana Economic History of Europe" series. An example of how sincerely Trebilcock attempts to keep the dialogue between description and theory going throughout the text is that in this chapter he not only comments on the possible applicability of Fogel's "let's imagine there were no railroads" analysis to Germany, but also presents (approvingly) Fremdling's counterdemonstration. Chapter 3, on France, is less successful, partly because those who insist on taking Gerschenkron's "spurts" of growth along with his "backwardness" will be somewhat confused as to whether Trebilcock believes France had any "spurts" at all. Students may be somewhat put off by the potentially bewildering exposition of both "orthodoxy" regarding some causal concept and its demolition in the same paragraph. Teachers, however, will appreciate Trebilcock's ability to shepherd his readers through this complex French material with the aid of pithy locutions and sarcastic observations, some of which are quite amusing. Since practically all of this book is based on other scholars' statistics and formulations, one's eyebrow raises occasionally at seeing how patronizing Trebilcock can sound when stating that so-and-so "rightly concludes" this or that; François Crouzet gets two such pats on the head on one page.

The chapter on Russia is solid and satisfying in that it brings us up to date on all the famous questions. But it also provides instances of how bewildered an unaided undergraduate could become. For example, in weighing the costs and benefits of emancipation, Trebilcock says "the intent of the reform was to avoid a second Pugachev,"

without any indication whatever as to what or who the first Pugachev may have been. Perhaps British undergraduates can be trusted to have had recent exposure to general Russian history (Trebilcock teaches at Cambridge University); my experiences with American undergraduates lead me to anticipate that successful use of this text will require instructors to perform heroic remedial tasks.

Chapter 5 is entitled "The Powers of Deprivation: Italy, Austria-Hungary, Spain." It is not up to the standards of the previous chapters; its treatment is thematic rather than nation by nation, so that it will be impossible for students to form a sequential picture of nineteenth-century development for each of these countries. This chapter, furthermore, underplays both the potentials and the achievements of western Austria-Hungary and northern Italy. And it ends rather weakly by concluding that for these three cases "economic success or failure seems to be geared unusually closely to the overall quality of government policy" (p. 379). This last theme is addressed again in the lively and ambitious concluding chapter, which suggests that a useful modification of Gerschenkron's model would take into account not only how entrepreneurs, financial institutions, and states attempt to compensate for backwardness but also how, occasionally, governments and some social groups attempt (deliberately or otherwise) actually to throttle growth.

Appended to this book are a score of carefully explained statistical tables and a highly useful, chapter-by-chapter bibliography. Although for American students, at least, this work has to be paired with a substantial account of British economic history, I feel Trebilcock should now be considered as "the" text in nineteenth-century European economic history.

MARTIN WOLFE
University of Pennsylvania

IVÁN T. BEREND and GYÖRGY RÁNKI. *The European Periphery and Industrialization, 1780–1914*. Translated by ÉVA PÁLMAI. (Studies in Modern Capitalism.) New York: Cambridge University Press or Éditions de la Maison des Sciences de l'Homme, Paris. 1982. Pp. 180. \$27.50.

Iván T. Berend and György Ránki are well known to readers outside their native Hungary, especially through their excellent *Economic Development in East-Central Europe in the 19th and 20th Centuries* (1974). The present volume appears in a series "devoted to an attempt to comprehend capitalism as a world-system" and sponsored by Immanuel Wallerstein and his colleagues at SUNY-Binghamton, an intellectual orientation that has determined the volume's title and conceptual organization, though not its

methods or conclusions. An extended commentary on the relations of the European "periphery" with the countries of the northwest European "core" provides Berend and Ránki with a framework within which they include a compressed but remarkably full discussion of the economic development of the arc of countries from Scandinavia through Russia, Eastern Europe, and the Balkans, and ending with Italy and the Iberian peninsula.

The level of discussion maintained by Berend and Ránki is uniformly high, and the results will be highly uncomfortable to proponents of single, overarching theories of development. Gerschenkron's theory of economic backwardness is subjected to a polite, well-mannered discussion that definitively demolishes its central contention of a correlation between economic backwardness and the level of state intervention (pp. 62–63). Wallerstein and the various "dependence" theorists fare little better, however, for Berend and Ránki show that the countries of the core did not simply exploit the periphery. Although relations between core and periphery were necessarily unequal, the interaction did not produce a single set of economic and social consequences.

The outcome of the complex interaction of core and periphery depended in each case on the peripheral region's previous history and the conditions imposed by the specific period of initial contact with the core. Essentially similar in their social and economic structures in the late eighteenth century, the regions of the periphery had become highly differentiated by 1914. Scandinavia had become part of the core, while Italy, Hungary, and certain regions of Russia were "on the road to thoroughgoing economic change." Other regions in southern and southeastern Europe "had hardly started . . . or were bogged down at the start." Berend and Ránki conclude that "we are, thus, left with no uniform scheme. . . . The integration of a more backward country did not necessarily and in all cases lead to its 'peripherization'; nor, however, was it necessarily an opportunity for catching up" (p. 160).

This conclusion, somewhat eclectic and agnostic on the surface, results not merely from citation of fortuitously diverse examples, but from a sustained theoretical analysis of carefully ordered comparative material. To Berend and Ránki it is clear that a single economic system was expanding to encompass new areas throughout the "long" nineteenth century. Berend and Ránki label this process "capitalist accumulation," and the only theoretical writer not subjected to their gently withering criticism is Lenin. Their Leninism, however, is undogmatic, even attenuated. The phrase "capitalist accumulation" could easily be replaced by, say, "modern economic development" without violating the structure of their argument—as long as it is kept firmly

in mind that economic development has a political dimension, both international and domestic, in the affected countries.

Berend and Ránkí provide an excellent and highly readable discussion of economic factors in the development of the European periphery. Population, capital flows, transportation development, international trade, growth of export industries, leading sectors, and their impact on the domestic economies of the countries of the periphery are all handled admirably, and their interrelations clearly delineated. The authors' touch, however, seems less sure when handling the political dimension. They include no systematic discussion of the international political system, and therefore their critique of "dependence" and "peripherization" theories remains incomplete in important respects. Their discussion of "the role of the state" is largely limited to listing and describing government policies in countries of the periphery. "The state" itself is not subjected to sufficiently rigorous analysis. Consideration of the sociology of government structures and elites, for instance, could have led to more satisfactory explanations of those policies, and perhaps to a more complete explanation of the differences in the peripheral countries' experience.

Although not without weaknesses, the book is an interesting, consistently intelligent attempt to analyze a broad and important topic. It will repay reading by anyone interested in European economic development.

FRANK B. TIPTON
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WOLFGANG J. MOMMSEN and WOLFGANG MOCK, editors. *Die Entstehung des Wohlfahrtsstaates in Grossbritannien und Deutschland, 1850-1950*. (Veröffentlichungen des Deutschen Historischen Instituts London, number 11.) Stuttgart: Klett-Cotta. 1982. Pp. 454. DM 88.

Both Great Britain and Germany were pioneers of the modern welfare state, although during different periods and in different ways. Social reformers and bureaucrats looked—and often enough came—across the Channel in either direction in order to inform themselves of the thinking and the measures taken abroad. The present volume, edited by Wolfgang J. Mommsen and Wolfgang Mock, describes various aspects of social policy in each country and offers some especially welcome discussion of the interrelationships between the two. The contributions are by historians, political scientists, and sociologists who participated in a symposium in Berlin in late 1978. The subjects and approaches, of course, are as diverse as the twenty British and

German authors, and there is little cohesiveness beyond the general theme. Because they span more than a century of pertinent developments, however, the essays provide a good introduction to the subject matter, and often much more.

The first group of essays deals with the period up to World War I. The British Poor Law is discussed by Derek Fraser, who gives a useful survey of present interpretations, and by Michael E. Rose, who argues that this law was transformed during the 1860s and 1870s under the influence of scientific philanthropy and should not be viewed solely as a failure when compared with later measures. As for the German side, Hans-Peter Ullmann endeavors to show that industrialists had a greater influence on Bismarck's insurance legislation than previously acknowledged, and Jürgen Tampke strives, although somewhat unconvincingly, to depreciate the breakthrough represented by this legislation; Anselm Faust gives a very solid and comprehensive account of governmental involvement with unemployment prior to 1918. Of the pieces examining German/British interaction, the ones by Roy Hay and Peter Hennock are noteworthy. Hay discusses the attitude of British businessmen toward the German insurance example but cannot find much consensus; there were strong groups who pulled in the German direction, but, most were motivated by economic interest. Hay not only does an excellent research job, but offers an array of sober insights and pertinent questions that are among the most thoughtful to be found in this volume. Hennock's subject is health insurance. He thinks that the British set-up was less bureaucratic than the German, but he believes that this benefited the commercial interests much more than the workers.

The most pressing social problem during the interwar period was unemployment. The essays of Michael Wolffsohn and, perhaps more cogently so, Bernd Weisbrod deal with the German experience in the last years of the Weimar Republic. While these authors cover some rather well-known territory, Robert Skidelsky's searching investigation of the competition between the conservative views of the British Treasury on unemployment policy and Keynes's unorthodox proposals is a sensitive discussion of a matter less frequently researched.

After World War II, the welfare state was finally inaugurated in earnest, first of all in Great Britain. José Harris, Roger Davidson and Rodney Lowe, and Jürgen C. Hess all look at different aspects of this occurrence. The most interesting piece of this section, however, is Hans Günter Hockerts's study of the influence of the Beveridge plan on postwar developments in Germany. Ironically, an Allied reform plan based on the British concept was abandoned by the Western powers but implemented in the Russian zone. In the long run, though, many of

Beveridge's ideas also filtered into West German thinking and influenced the social policies of the Adenauer era. Of the remaining essays only Peter Flora's attempt at assessing the welfare state's potential as a cause for crisis deserves mention here. It is a pity, however, that he does not try his model on the verifiable past but prefers to apply it to the speculative present only.

As Hay writes in his contribution, we still lack a clear concept capable of explaining why countries different in tradition and social structure arrive at certain social policies that bear remarkable similarities. This volume clearly does not offer such a concept either. By pointing out some of those similarities, and of necessity also some differences, it nevertheless constitutes a welcome step on the way to the ultimate goal.

UDO SAUTTER
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R. H. CAMPBELL and A. S. SKINNER. *Adam Smith*. New York: St. Martin's Press. 1982. Pp. 231. \$25.00.

Adam Smith is a difficult subject for a biography. He was always a bad correspondent, and he insisted that his executors burn sixteen volumes of manuscripts after his death. Furthermore, any biography has to be based on an understanding of Smith's major works and also of the academic, intellectual, and administrative environment in which he lived. Fortunately, R. H. Campbell and A. S. Skinner are remarkably well qualified to undertake the task of writing Smith's biography. They are experts on Scottish economic and intellectual history, and they are engaged in the Glasgow edition of the *Works and Correspondence of Adam Smith*. Their biography is aimed at those who know comparatively little about either his life or his ideas. It is neither a detailed nor a thoroughly documented study, but as an introduction to the man and his work it is a very considerable achievement. It is based on extensive knowledge, and it is remarkably clear and succinct.

The authors know even more about Smith's works than about his career. Perhaps because of this, or because of the sheer complexity of Smith's ideas, their chapters on his writings are more congested and less successful than the narrative chapters. Although there is relatively little known about Smith's life and career, the authors succeed in building up a very clear narrative. They provide a brief, but lively and intelligent, account of Smith's achievements as a Glasgow University professor, as a private tutor, and as a commissioner of customs in Scotland. They succeed in bringing Smith to life in his friendships, his relations with his publishers, and in his academic and business pursuits. In the process they bring out two very interesting points about the

relationship between Smith's life and works. Smith acquired a considerable knowledge of Glasgow's trade, which prospered because of the mercantilist restrictions of the Navigation Acts, and yet he later wrote the greatest condemnation of such trade restrictions. Moreover, even after he had launched his attack on the iniquities of the complex network of customs duties and regulations, he was prepared to accept an office as a commissioner of customs. His experience helped him to strengthen his criticisms of mercantilism in the third edition of the *Wealth of Nations*.

The chapters on Smith's writings are less successful, though they do provide a useful introduction to Smith's major ideas and demonstrate Smith's remarkable intellectual range. The summary of Smith's writings on rhetoric, morality, and jurisprudence is clear and fairly comprehensive. Much less so is the chapter on the *Wealth of Nations*, though the authors do provide a useful discussion of Smith's views on Britain's relations with the American colonies. In tackling Smith's writings work-by-work the authors make it difficult to get an overall view of his distinctive style of thinking. These are minor faults, however, in a work that can be confidently recommended to all those who need an introduction to Adam Smith.

H. T. DICKINSON
University of Edinburgh

NEIL MCKENDRICK *et al.* *The Birth of A Consumer Society: The Commercialization of Eighteenth-Century England*. Bloomington: Indiana University Press. 1982. Pp. viii, 345. \$29.95.

The essays in this uneven volume proclaim the importance of the commercial revolution that occurred in England during the second half of the eighteenth century. The authors contend that the consumer society emerged then and there, and that the Industrial Revolution cannot be properly understood without the enormous expansion of the home market that accompanied it.

The authors are less concerned with the reasons for that expansion than with the evidence for a public avid to purchase material objects. They range far afield in their examples and devote considerable space to the ingenuity, the entrepreneurial energy, revealed in the advertising and marketing of these goods—whether they be Wedgwood vases, razor strops, Wilkite medallions, or jigsaw puzzles. The economic influence of fashion, the social effects of snobbery, and the central part played by social emulation in stimulating increased spending are recurrent themes throughout the essays.

Neil McKendrick's contribution occupies nearly

two-thirds of the volume, to the book's detriment. His essays—on the commercialization of fashion, of the potteries, and of shaving—suffer from extreme repetition. Not only does he mercilessly repeat the main points, such as the magnitude of the commercial revolution in eighteenth-century England, but he even uses specific quotations more than once. We are told no less than three times of the “‘violent vase madness’” in Ireland, for example (pp. 76, 100, 117). A self-congratulatory tone, furthermore, mars his criticisms of fellow historians who have not appreciated the significance of the material he examines. He chides his professional colleagues on many counts, but particularly for failing to explore the demand side of the Industrial Revolution as fully as the supply side, for neglecting Josiah Wedgwood's and Matthew Boulton's activities as commercial hucksters, and for ignoring the important role of the small-scale entrepreneur, like George Packwood, inventor of an improved razor strop.

McKendrick's chapter on Packwood is the most disappointing in the volume. It was an excellent idea to focus on an unknown businessman and to reveal, through his ingenious marketing strategy, that the consumer revolution rested on the achievements of little men as well as on giants like Wedgwood. Yet McKendrick lacks sufficient information about Packwood to build a credible argument. Nearly thirty pages of quotations from advertisements for shaving gear tell us nothing about the extent to which Packwood captured the market for razor strops. Nonetheless, McKendrick takes the endless puffery for proof of Packwood's success and concludes that Packwood belongs among the unsung lesser businessmen who contributed to England's “rapid economic progress” (p. 193).

J. H. Plumb's three essays also contain considerable repetition, as he draws his assertions concerning the commercialization of leisure, the commercial exploitation of childhood, and the growth of modern attitudes toward nature from a restricted body of evidence, particularly children's literature. Yet the argument developed in his final essay—“that quite humble activities played their part in the acceptance of modernity and of science” (p. 333)—is important. It fittingly complements another of the volume's principal contentions, that the Industrial Revolution depended heavily “on the sales of humble products to very large markets” (p. 53). Great changes occur through modest means.

The shortest contribution to the volume, John Brewer's single essay, develops with exemplary clarity a complicated argument concerning the bonds between eighteenth-century commercialization and radical politics.

JANET OPPENHEIM
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JOHN A. PHILLIPS. *Electoral Behavior in Unreformed England: Plumpers, Splitters, and Straights*. Princeton: Princeton University Press. 1982. Pp. xix, 353. \$35.00.

John A. Phillips has produced a brilliant study of borough politics in the latter half of the eighteenth century. It is undoubtedly one of the most significant works since those of Sir Lewis Namier and is based on an exhaustive analysis of nominally linked records derived from pollbooks, tax returns, and religious registers of four typologically selected boroughs (Norwich, Maidstone, Northampton, and Lewes) during the years from 1761 to 1806. His thesis is that a fundamental shift occurred from the nonpartisan, apolitical borough elections of the mid-eighteenth century to highly partisan party politics by the end of the century. By the 1780s the electorate of the more popular franchise boroughs, which comprised the majority of the unreformed borough electorate, had become intensely politicized and issue oriented and displayed strongly marked secular patterns of persistent and consistent straight party voting. But the entire borough electoral system displayed signs of increasing conflict, despite substantial increases in patronage and a continuation of minimal corruption. The electoral conflict within boroughs was focused by the emergence of local party organizations by the 1780s. Party organizations emerged most strongly (and perhaps only) in those boroughs whose municipal charters permitted a wide municipal franchise with frequent municipal elections. Municipal and parliamentary politics were thus inextricably linked, first, through the structural conditions of politics established by municipal government and, second, through the political orientation of local party organizations. By the 1780s local parties had become publicly associated with the issues espoused by the two national political parties. Most astonishingly, Phillips is able to establish statistically that political conflict in his boroughs was substantially unrelated to social or economic differences within the electorate: religious differences, the only significant determinant of party preference in the boroughs he examined, directly stimulated party conflict and may have been similarly important in as many as sixty of the larger unreformed constituencies. He suggests tentatively that the politics of late unreformed England might best be characterized as “religiously-colored,” giving way to “class-dominated” politics at some unknown point in the nineteenth century.

Whether late unreformed politics may be broadly characterized as religiously colored remains to be seen. It is also difficult to accept, despite the Norwich statistics, that local Dissenters favored the Portland Opposition over the government and person of Pitt during the early 1780s. The important

recent work by Cookson, who suggests a strong break with Pitt and the convergence of the Dissenters with the Foxite Opposition occurred only in the 1790s, must be reconciled with Phillips's findings. Phillips is also unable to establish unequivocally his crucial linkage between local and national political parties, since he insists on viewing national parties after 1780 as ideologically homogeneous, or at least sufficiently so, which they certainly were not before 1794. They were especially divided on those issues that were dearest to the hearts of Dissenters. Such issues were in fact considered nonparty concerns in the parliamentary parties of the 1780s. Portland himself favored the Test and opposed parliamentary reform. But while I find myself quarreling endlessly with Phillips on specific points of interpretation, the larger thrust of his interpretation, with some modifications, is thoroughly persuasive. It should focus debate on the unreformed political system for some time to come, and that is a very high compliment.

DONALD E. GINTER
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EDWARD ROYLE and JAMES WALVIN. *English Radicals and Reformers, 1760–1848*. Lexington: University Press of Kentucky. 1982. Pp. 233. \$18.50.

Ambition is welcome in writing history and this is an ambitious book. It sets out to describe the views of those who, between 1760 and 1848, "sought to institute a reform of the political system in a democratic direction" (p. 10) and attempts to do it in 192 pages. The scaling of Mount Everest over a weekend would hardly present fewer problems. For a fast-moving and manageable account of an infinitely complex subject, this book is liable to become the student's friend. It will lead him painlessly from generalization to generalization, without unduly misleading or greatly enlightening.

Breathlessness is everywhere. There was clearly little time for proofreading. The existence of a "Duc de Rochefoucauld" and a "Mirabaud" would surely surprise the Paris Jockey Club, and the idea that Doumouriez was leading an emigre government in the spring of 1792 is not to be accepted unquestioningly. Such is the pace, too, that there is little time for the precise defining of terms. Foxite Whigs were not unduly fussy about the company they kept, but to indicate that they were "middle-class parliamentary reformers" (p. 77) would not have won admission to Brooks's. "Tory" is a word that should not be exhumed by the unwary, and "Radical" offers a positive minefield of possibilities. To use it of those who campaigned against the slave trade and of those who looked to a change in the nature of

representation is to blur the images of Wilberforce and Major Cartwright.

Inevitably, given the constraints under which this book is written, the presentation is rather episodic. From Wilkes to Wyvill, through Blanketeers and the Queen Caroline Affair, to the Reform Bill and Chartism the long march is undertaken. Without offering much that is new, Edward Royle and James Walvin attempt at each of these stopping places to bring the reader up-to-date on recent scholarship and to make him aware that he is only being introduced to a subject, the mastery of which will involve much sweat. At times the authors almost despair of achieving their goal, so steep is the rock face: "To write of radicalism in the 1830's is especially difficult, for there are so many conflicting streams not really identifiable with class or party" (p. 157).

Yet valuable themes are brought out. In this period, the function of reform programs changed from being designed to protect and make more effective government by the propertied to being directed toward more democratic notions. In this process, the old radicalism incorporated developing class attitudes and was enriched by them. It was not, however, subverted by them. Radicalism remained a broad church, and the hope is to chronicle "the steady expansion of radicalism's establishment" (p. 191). This is a noble theme more suited to the marathon runner than the sprinter.

LESLIE MITCHELL
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Oxford University

CHARLES F. BAHMUELLER. *The National Charity Company: Jeremy Bentham's Silent Revolution*. Berkeley and Los Angeles: University of California Press. 1981. Pp. xi, 272. \$25.00.

At a time when historians, inspired by the work of Michel Foucault, Erving Goffmann, and others, are showing a renewed interest in the role and development of institutions, this book is a timely one. Jeremy Bentham's pauper panopticon, as described in his *Pauper Management Improved* of 1797, was perhaps the most total of total institutions. Within its walls, beggars and vagrants could be profitably and productively employed; pauper children could be apprenticed and educated in civic duties while contributing by their labor to the profits of the enterprise; the sick poor could have their habits of industry maintained while being treated in the infirmary; and insurance could be sold through the institution to those of the independent poor who wished to guard against having to enter the panopticon poor house.

Thus the panopticons, administered centrally through a National Charity Company modeled on

the East India Company would be national engines of regeneration rescuing England from the crises of high food prices, rising poor rates, and political unrest into which inadequate government and Jacobin ideas had thrown it in the 1790s. By internal colonization of surplus labor into the panopticons, indigence could be made profitable and prosperity created through the principle of inspection. Yet, like so many of the utopias and pantisocracies of these fertile decades of the Industrial Revolution, Bentham's scheme remained a proposal on paper only. Despite reprints of his pamphlet in French in 1800 and in English in 1812, Parliament rejected both his prison and his pauper panopticon schemes. Generous monetary compensation notwithstanding, Bentham was bitterly disappointed. "J. B. never thinks of Panopticon without grief," he wrote. The scheme, in Charles F. Bahmueller's words, became Bentham's "trail of tears" (p. 58).

The early chapters of the book set Bentham's ideas clearly within the context of the poor relief crisis of the 1790s. *Pauper Management*, unlike William Pitt's abortive bill of 1796 or the Speenhamland decision of 1795, seemed to permit a final solution to the problem of poor relief by applying principles of profitability and rational management while retaining collective, communal responsibility for the poor. Bahmueller shows Bentham energetically lobbying for his proposal and attempting to persuade such eminent contemporaries as William Wilberforce, Arthur Young, Sir Frederick Eden, and Benjamin, Count Rumford of its great potential.

In the later chapters, which discuss the scheme in detail, some of this sense of historical perspective is unfortunately lost. At times, Bahmueller seems to stand rather too close to his subject. As a result, he becomes emotionally entangled in it, waxing indignant at Bentham's careless regard for humanity while admiring him as the "first important systematic defender of the Welfare State" (p. 214). This concept of the welfare state threatens to become Bahmueller's trail of tears. Although he points out early in the book that "this 'Welfare State' of Bentham's, however, was not the modern Welfare State as we know it" (p. 2), the anachronism in full capitals confronts the reader at every turn. While recognizing that it is (saving only the BBC and the Royal Family) the British institution most coveted by Americans, its omission here would not have harmed the argument. Fewer clichés ("when Bentham heard the words 'increased costs,' he reached for his gun" [p. 121]) and chatty rhetorical questions ("What then were the facts? What indeed?" [p. 113]) would also have been welcome. The absence of illustrations, given the intriguing cover design, and of a comprehensive bibliography is a puzzle.

Despite these quibbles, however, the author has given us a thorough interpretation of a major work

by a crucial, perverse, and perplexing thinker. This is an important contribution to the history of social policy.

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WILLIAM L. PRESSLY. *The Life and Art of James Barry*. (Studies in British Art.) New Haven: Yale University Press, for the Paul Mellon Center for Studies in British Art. 1981. Pp. xiii, 320.

This is the first monograph to be published on James Barry (1741–1806), that irascible (not to say, paranoid), heroically dedicated painter of historical painting. Barry has not lacked champions and commentators (Bodkin, Waterhouse, Wark, Irwin), but William L. Pressly now offers a full life-and-works study, with some detailed analyses of Barry's imagery and style as well as catalogues of paintings, drawings, and prints.

Barry's career began modestly in Cork, continued more ambitiously in Dublin with a painting (recently identified) of St. Patrick baptizing the king of Cashel, and then progressed—with help from Edmund Burke and his brother—in London, Paris, and Rome, which he reached in 1766. Italy saw both fine examples of his ambitious painting (a self-portrait, now in the National Portrait Gallery, London, *The Temptation of Adam, a Philoctetes*) and early manifestations of his inability to get on with patrons and fellow artists.

Back in London in 1771, Barry faced not only the challenge of rival artists, like Benjamin West or Nathaniel Dance-Holland, but the more intractable problem of securing patronage for the "high-minded" art to which he aspired. He exhibited at the Royal Academy, where he became an associate in 1772 and a full royal academician the following year. But his hopes for a major commission to decorate St. Paul's were disappointed, and this provoked the first of several literary appeals on behalf of historical painting, *An Inquiry into the Real and Imaginary Obstructions to the Acquisition of the Arts in England* (1775). There followed some years of fine portraits and classical subjects, one from Milton (always Barry's favorite sublime writer) and *The Death of General Wolfe*, deliberately composed to vie with West's work six years before.

His great opportunity came with the series of paintings in the Great Room of the new Adam building in the Adelphi, the home of the Society for the Encouragement of Arts, Manufactures, and Commerce. This ambitious project, nourished by Barry's literary as well as painterly interests, is his *chef d'oeuvre* and appropriately occupies a substantial section of Pressly's book. But it is precisely here that Pressly's rather unexciting, largely descriptive ap-

proach to the paintings seems most inadequate. Barry is surely among the most intriguing exponents of *ut pictura poesis* at a time when such traditions came under fire (notably from Lessing) or were much revised by others like William Blake. Yet Pressly, despite the claim that Barry's "career reveals much about the attitudes of eighteenth-century England towards art and artists" (p. 1), simply does not rise to the challenge of discussing this aspect of his subject. The place that might be claimed for Barry in the shifting perspectives of late eighteenth-century British art—not just his formal parallels with other artists, but his deeper attitudes toward painting and its literary and psychological dimensions that link him with Henry Fuseli, Runciman, or William Blake—is only hurriedly sketched in a five-page conclusion.

The Life and Art of James Barry is a welcome book, however, and will doubtless prove a standard survey. Pressly is sympathetic to his tetchy and disagreeable subject. He tactfully narrates Barry's feud with and expulsion from the Royal Academy and his neglected last years. There is also some useful commentary on Barry's political attitudes and the often radical propaganda to which he devoted some of his prints.

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ROBERT BLAKE. *Disraeli's Grand Tour: Benjamin Disraeli and the Holy Land, 1830–31*. London: Weidenfeld and Nicolson. 1982. Pp. xvi, 141. £8.95.

Here is a book to savor. Gracefully written, it puts together an account of Disraeli's travels to the Near East as a young man in his mid-twenties seeking to recover from a nervous breakdown. The materials for the story come largely from Disraeli's own sparkling pen in the form of letters back to his family, supplemented by recollections in his later novels. Robert Blake, calling on a lifetime of wide reading and incomparable knowledge of Disraeli, weaves brightly colored extracts from the letters and novels together with quiet prose like the background in a Turkish rug. Suggested by Lord Weidenfeld, the book was written by Blake after six weeks "as a guest in Mishkent Sha'ananim, commanding a splendid view of the Old City" of Jerusalem (p. xv).

Gently the book proves to be more than the tale of a flight on a magic carpet by the most exotic of Victorians. Blake threads in his own reflections as an English Conservative. He reflects on the moral ambiguity of the Greek revolt against Ottoman rule, which liberals then and since have tended emotionally to oversimplify, and at the same time on the

greater efficiency of democracies over autocracies, which even liberals have sometimes doubted.

Blake saves his commentary on the significance of the tour for Disraeli as a person and politician until a final chapter entitled "Aftermath." The pride that Disraeli took in his Jewishness has long been recognized. Right after his return from the Near East he wrote the novel *Contarini Fleming* in which he pointed out that while his race was writing the Old Testament "the inhabitants of England were going half-naked and eating acorns in their woods." His travels through Greece, barbarized by revolt and repression, to Constantinople, through Syria and, above all, to Jerusalem pumped blood into his imagination. But he attempted to convey the impact that his discovery of Jerusalem and the Ottoman empire's Jews had on him in such a highly colored, conceptually opaque manner that contemporaries and later votaries never knew quite what to make of it.

Blake does not claim to dispel all the mystery. When Disraeli in his novel *Tancred* has an angel announce "the sublime and solacing doctrine of theocratic equality," Blake comments that it "may be sublime and solacing, but it is anything but clear" (p. 124). Blake manages, nevertheless, to dispose of some possible but erroneous impressions, for instance that Disraeli's concern was to protect himself against antisemitism. Blake confirms what he has already suggested in his great biography of Disraeli, that in his Jewishness Disraeli found credentials for himself as a natural aristocrat and confirmation of his belief in aristocracy. In this new book, Blake roots Disraeli as a person firmly and convincingly in his confirming encounter with Jerusalem and the Syrian Jews in 1830–31. Sensitive from adolescence to being different, and anxious not simply to be accepted but to rule, he needed to create for himself a persona and a compensatory myth. He found it in the Holy City, though, as always, in a strange way, the product more of romance than of reality. Blake concludes by suggesting that within the later pragmatic statesman lay a latent Zionist.

PETER T. MARSH
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T. W. HEYCK. *The Transformation of Intellectual Life in Victorian England*. New York: St. Martin's Press. 1982. Pp. 262. \$25.00.

This is a stimulating book that presents its arguments with exemplary clarity. T. W. Heyck's aim is to explain why Victorian England had no specific class of intellectuals and how it was that by the end of the century it had acquired one. He begins by examining the close relationship between the educated Victorian public and its men of letters, schol-

ars, and men of science. He then argues that this relationship was undermined by the growing claims of natural science. To escape the pressures of the marketplace, the natural scientists wanted more money and higher social status. Fiscal orthodoxy ruled out much help from government; and, since Oxford and Cambridge had both the funds and the social prestige, it was to them that the scientists looked. Their unreformed state was the main obstacle to recognition of the claims of the scientific community, which accordingly provided the main motive force and set the pace of university reform after 1850. Heyck illustrates the process by which scientific inquiry extended its dominion in a chapter on the development of historical scholarship from Thomas Carlyle and Thomas Macaulay to Edward Freeman and Frederic Maitland. But the success of science had its price. In gaining a coveted place in the genteel world of "Oxbridge," the scientists lost sight of their old utilitarian purpose and were infected by the elitist notion of the pursuit of knowledge for its own sake. They also broke up the "common context" of natural theology that earlier in the century had given diverse disciplines a sense of common purpose. The resulting fragmentation spread disillusionment and dismay through the world of intellect, which made it unfit to deal with the new mass public that developed after 1870. Some joined with Matthew Arnold to call for a high culture fenced by academic exclusiveness. Others retired with Walter Pater into an etiolated aestheticism and the ideal of art for art's sake. The intellectuals were the product and the expression of this alienation from popular concerns.

There is much food for thought in all this, but the argument has serious flaws. There is a conceptual muddle about what constitutes an intellectual that runs right through the book. Heyck seems to assume that the coining of a word is proof of the existence of the thing, although "intellectual" as a noun describing a class is a French coinage and not the first such term to enter the language well ahead of the social change it purports to describe. Heyck skillfully describes the changes on the literary scene and in the universities in the third quarter of the century (chapters 3 and 4 on the impact of science being especially good), but he is more impressionistic on the final decade, where his account of the intellectuals' alienation relies heavily on Walter Pater and Oscar Wilde. As if aware that these figures may not be wholly representative, he admits that the early socialists were an exception to the alienation of the man of letters from the general public; but that does not take us very far into a literary world that included Thomas Hardy, Joseph Conrad, Rudyard Kipling, and Arnold Bennett.

This relates to what seems to me the central fault of the book and indeed of this whole genre of

intellectual history, that is, it is weak on the social and political context. Though he mentions France and Germany, Heyck has missed the central fact of English society that makes England so unlike other European countries—it has always (till very recent times) absorbed its educated classes into its political system. He consistently underrates, or ignores, the political functions of the writers and thinkers he examines. It is significant, for instance, that he deals hardly at all with that quintessential expression of the bourgeois mind, the political economy, even as an aspect of the spread of the positivist outlook. His account of the professions in Victorian England ignores the most political of all, law; and his treatment of society leaves out the most useful applied science, medicine. Even in stressing the prevailing clericalism of Oxford and Cambridge, he discusses the clergy's role in English culture without mentioning their functions in local government. They are represented as engaged in futile and unworldly scholarship, like George Eliot's Casaubon or Anthony Trollope's Josiah Crawley. Most seriously, Heyck gives only a glance at a vital episode in the development of the Victorian "clerisy," the opening of the civil service to competitive examination, a change that saved the ancient universities from being mere clerical corporations and turned so many of their graduates not into intellectuals but into something arguably more useful, civil servants. Here Trollope's Sir Gregory Hardlines would have repaid study more than Josiah Crawley. In short, how men develop and apply their talents is only part of the story: we need to know also what opportunities their society affords them and why. Heyck should have written a longer book or else devised a more modest title.

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DENNIS SMITH. *Conflict and Compromise: Class Formation in English Society, 1830–1914: A Comparative Study of Birmingham and Sheffield*. Boston: Routledge and Kegan Paul. 1982. Pp. xiii, 338. \$39.95.

I suspect that Dennis Smith's book will delight the hearts (and minds) of that new breed of urban sociologists, geographers, and social historians for whom the comparative and neo-Weberian study of limited aspects of small regions is the key to real knowledge. And I have no doubt that Smith's conclusions, only loosely based on the discussion of the peculiarities of the histories of education in Birmingham and Sheffield between 1830 and 1914, which is the core of the book, will be welcomed as additional indisputable falsification of the materialist conception of history. Thus the book concludes

that capital accumulation and the reproduction of capitalist relations of production have no necessary priority in cultural (educational) and political determination. And it claims, weakly and as if contradicting some central tenet of the materialist conception of history, that the development of education, industry, and local government "was an aspect of transformations within the broader framework of social differentiation" and that the composition of social classes and consciousness were products "of the complex intermeshing between the old society and the new."

Such weak conclusions, unaccompanied by any reconstruction of life in the two cities, are a poor reward for struggling with the awkward presentation of the findings of "the comparative method"—a sentence or a paragraph or two on Sheffield followed by a sentence or two or another paragraph on Birmingham—and with a quasi-sociological language about limited aspects of the two cities that says less about them than, say, Pollard on Sheffield and Briggs on Birmingham. Further, it is surprising that a book that purports to say something important, if not profound, about the relationships between economy, social structure, and aspects of culture refers the reader to only one work by Raymond Williams, and that to a book he claimed recently to have left behind!

In short, while *Conflict and Compromise* has something to say about class formation in English society (in Birmingham and Sheffield), it has little to add to the discussion on class consciousness (as an aspect of class formation), and it makes no contribution to the general debate about the relationships between economy and culture. The book is probably most useful as a contribution to the history of education in both cities and, therefore, as a contribution to some future theoretically structured and readably constructed histories of those cities.

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STANLEY R. STEMBRIDGE. *Parliament, the Press, and the Colonies, 1846–1880*. (Modern British History.) New York: Garland. 1982. Pp. 310. \$35.00.

This study of Victorian opinion emerges from patient delving into parliamentary debates, memoirs and biographies, and, most of all, files of newspapers and magazines. Much interesting and some amusing material is turned up from under the soil, and the writing is lively, with a distinct turn for epigram. Gladstone's speech on the New Zealand Bill in 1852 is cited as "an example of his talent for convincing himself of anything whatever" (p. 70). It

would have been worthwhile to make it clearer at the outset that the colonies under discussion are the areas of British settlement. Britain had two different empires, though sometimes, especially in southern Africa, they merged into each other.

Stanley R. Stemberidge begins by pointing out the tenacity of an old belief that until 1860 British sentiment was in favor of turning the colonies loose and that after that date separatism was speedily displaced by imperialism. His thesis is that this is a misconception. Dislike of colonial connections was strongest before 1846; a critical "period of adjustment" in relations between 1846 and 1854 ended in a "remarkable transformation" in favor of imperial unity (pp. 46, 81), though feeling remained liable to frequent fluctuations. These shifts affected individuals as well as the public, at least the part of it that thought about the subject at all. Tories were most often to be found championing not only retention of the colonies but firm control over them; but their ranks, like those of the Liberals, were not always united. There was a prevalent view, which must have owed much to memories of the American Revolution, that, although Britain would be happy to see the colonies keeping up their links with it, Britain must not try to coerce them if they wanted to go their own way. The *Times* was typical in its proneness to sudden shifts and fits of irascibility and in its concern for John Bull's pocket. "It desired imperial greatness, but did not want to pay for it" (p. 88).

Questions of defense costs and of how big a part of them the reluctant colonists ought to be made to bear always loomed large. In the case of Canada they were compounded by the proximity of the U.S. and the ambition of many Americans to swallow Canada up. Any prospect of a war with the U.S. was daunting and made a good many statesmen and others wonder whether it would not be prudent to let Canada go. New Zealand with its Maori wars and Cape Colony with its still more chronic Kaffir wars always wanted Britain to provide the troops and their pay, while the settlers reaped the reward. Mixed with dislike of this arrangement was some sympathy in Britain for the native peoples. But when it was decided to withdraw British forces from New Zealand, leaving the colonists to do their own fighting, this meant also leaving them a free hand with the Maoris: "economy was entirely victorious over philanthropy" (p. 181).

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JEFFREY COX. *The English Churches in a Secular Society: Lambeth, 1870–1930*. New York: Oxford University Press. 1982. Pp. xii, 322. \$37.00.

Jeffrey Cox's study of church life in the London borough of Lambeth from 1870 to 1930 is a valuable addition to the number of local studies in modern English church history. Cox makes good use of the local press and the notebooks from Charles Booth's massive survey of London religion. In addition to worship, close attention is paid to the variety of other services offered by the churches to the community—poor relief, thrift societies, medical care, education, clubs, popular recreation, and entertainment.

But the book is also a significant contribution to the wider historical (and sociological) debate about the role of religion in modern society. Cox is critical of all loose talk about "the decline of religion." Eschewing secularization as a term that is at best descriptive and at worst tautological, he seeks historical explanations for the changing role of organized religion in the period. These are found at two levels. Generally, the modern period brought the final breakdown of attempts to impose religious unanimity on the nation by means of a state church, and the resulting pluralism allowed the freer expression of religious indifference or unbelief. In England particularly, the late Victorian and Edwardian periods represented an accelerating takeover by local authorities and the state of many social-service functions hitherto performed by churches, inevitably confining the churches to a more narrowly religious role.

A key group in this transition was the Nonconformists, whose numerical decline after 1906 was sharper than that of the Church of England. Ironically, the Nonconformists were often champions of wider responsibilities for secular authorities in, for example, the provision of public baths and libraries, and ultimately the replacement of private benefit societies by a national system of welfare insurance. Sometimes, indeed, the secular authorities were championed precisely because they displaced the established church, as in education. Nonconformist numerical decline, however, was due to the movement from a "conversionist" ethos to a "civilizing" one (in which the churches' role is less important), combined with a longstanding failure of many Nonconformist parents to hold the allegiance of their children for the chapels.

The churches had less leverage on social problems in the 1920s because, as they ceased to be responsible, their views counted for less. Moreover, the people who made vital social policy decisions were less likely to be religious. "No one stepped forward to replace Lambeth's late Victorian religious activists, the men and women who worked the parochial machinery and urged both their friends and their clients to attend church" (p. 211). Why? Cox argues that Edwardian churchmen had nothing to say to the outside world. This is too simple.

While it is true that "conversionist" churches declined more slowly than others in the twentieth century, much effort was put into evangelistic activity after 1920, especially by Nonconformists, that has simply been fruitless. The failure of revivalism is also part of what has to be explained. Nevertheless, this is a fascinating book that deserves a wide circulation.

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NORMAN MACKENZIE and JEANNE MACKENZIE, editors.
The Diary of Beatrice Webb. Volume 1, "Glitter Around and Darkness Within," 1873–1892. Cambridge: Belknap Press of Harvard University Press. 1982. Pp. xxii, 386. \$25.00.

Beatrice Webb published *My Apprenticeship* in 1926. Subsequently, *Our Partnership* was posthumously brought out under the editorial supervision of Barbara Drake and Margaret Cole in 1948. In composing these works, Webb drew on the contents of a vast manuscript diary she had begun to keep on a more-or-less regular basis in September 1873, when she was just fifteen, and in which she continued to write for some fifty years. The final entry was made on April 19, 1943, only eleven days before the end of her long and eventful life. The first of these two autobiographical works, the much-acclaimed *My Apprenticeship*, written in narrative form with periodic recourse to direct quotation from the diary, covered her childhood and early intellectual formation and ended with her marriage to Sidney Webb in 1892. The sequel volume continued the story down to 1911, when the now-famed couple departed England for a year-long world tour. Beyond that, Webb planned at least one further volume of autobiography; but with the onset of old age and recurring ill-health, she simply added, in 1938, a brief postscript to the virtually completed draft of *Our Partnership* intended to justify the Webbs' controversial conversion to Soviet communism late in life. During the 1950s, Margaret Cole undertook to fill in the gaps by producing two further volumes based on the diary, which covered the periods 1912–24 and 1924–32 respectively. Separate volumes devoted to the Webbs' travels in New Zealand, the United States, and Australia made their appearance, too, in due course.

Notwithstanding the previous publication of these substantial extracts from the diary (and of Norman MacKenzie's three volumes of *The Letters of Sidney and Beatrice Webb* [1978]), very little of the personal material in the diary—which runs to some two million words—and none of the materials from the final ten years have ever been published. Since 1978

researchers, at least, have benefited from the availability of a microfiche of the complete holograph diary accompanied by a typewritten version of it. But now, with the appearance of the handsomely produced volume here under review, we have as well the first installment of a larger undertaking that promises to make accessible to a larger audience for the first time in book form what Norman MacKenzie and Jeanne MacKenzie suggest is "an overall and systematic edition of the diary" (p. xxi). A second volume (1892–1906) is planned for 1983, a third (1906–14) for 1984, and a fourth and final volume (1914–43) is to appear in 1985.

The original diary, in Webb's barely decipherable hand, is contained in fifty-seven exercise books now housed, along with two typescript versions of it, in the library of the London School of Economics. In preparing the first of their projected volumes, the MacKenzies have relied primarily on one of the typescripts, partly made by Webb herself, of the first fourteen of the hand-written exercise books, referring back to the originals whenever her meaning appears uncertain. While they have had to make substantial editorial cuts, their aim throughout has been "to preserve the spirit, the spontaneity and the substance" (p. xxi) of the uncut diary.

The period of Webb's life covered by this first volume corresponds directly with that of *My Apprenticeship*, though here, aside from the MacKenzies' accompanying editorial matter, the diary is allowed to speak for itself rather than through Webb's superimposed and retrospective narrative. Following a general introductory essay, the editors have divided the material into five parts. The first part covers Beatrice Potter's earliest years and the formative influence of family and family friends, foremost among the latter Herbert Spencer and Francis Galton, whose intellectual example awakened vague aspirations in a privileged yet purposeless young woman. Next, there is a section devoted to the emotionally draining period of her beloved father's lingering illness and her deeply disorienting and unrequited passion for the Liberal politician Joseph Chamberlain. Then we observe Potter, working her way out of what she called the dead point of her career, as a social investigator assisting Charles Booth with his great survey of London's poor. There follows an account of Potter's first publications and public recognition based on her East End experiences as a rent collector and a disguised trouser hand in a sweated shop, these signaling her arrival at a firm sense of vocation, of being at last a truly independent woman with a dawning interest in the cooperative movement and the trade unions. Finally, the editors pass to her eventual introduction to Sidney Webb, the fellow brain-worker who encouraged her conversion to socialism and became her seemingly implausible

suitor. The volume closes with their marriage at St. Pancras Vestry, London, on July 23, 1892, and this poignant entry in the diary: "Exit Beatrice Potter. Enter Beatrice Webb, or rather (Mrs.) Sidney Webb for I lose alas! both names" (p. 371).

The MacKenzies suggest in their introduction that in the case of Beatrice Webb, née Potter, "there were, in effect, two partnerships. One was the life-long colloquy with the diary, which kept her sane. . . the other was with Sidney, who kept her effective" (p. xix). Anyone familiar with the contents of the original diary, or indeed anyone who reads this volume of selections from it, will find little to quarrel with in that assertion, though the editors' suggestion that the laudanum bottle was never far away from one so strong-willed may appear a trifle on the dramatic side. What emerges clearly in this volume is the distance separating Webb's assured and even imperious public persona—"A rather hard and learned woman, with a clear and analytic mind" (p. 232), according to the journalist H. W. Nevison at the time—and the anxious and self-searching private persona of the diary. In her restless yearning for answers to life's big questions, the editors clearly note that Webb had the makings of "a great *religieuse*," but that the circumstances of her life made her a great social scientist instead. Be that as it may, one cannot help but wonder, reading here of her youthful literary ambition and on the strength of the achievement of the diary itself, what a talent was lost to general letters by her distraction with questions of social and political science.

It is perhaps worth noting that in England this volume is published by the feminist house, Virago Press. The MacKenzies, while noting that Webb was in no sense a conventional feminist (she was more interested in class and the profit motive than sexual gender, and besides, as she tells us in *My Apprenticeship*, the circumstances of her station conferred on her a pervasive sense of her privileged position), are of the view that "one sees how much her struggle to find herself was, in some measure, a woman's struggle in a man's world, and that a diary which begins as an intensely personal document can become a testament for a whole generation of women" (p. xx). Doubtless there is something in this. Occasionally the editors are tempted, out of an admiration for Webb's achievement, to abandon their critical distance, as in the claim that she had "all the attributes of a successful politician and the manner of a Cabinet minister" (p. 317). But this is an observation rather than a criticism of a welcome and long overdue edition of the diary of Beatrice Webb. It is a compelling read, making available to students of the period a window on the late Victorian world.

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LESLIE PARKER HUME. *The National Union of Women's Suffrage Societies, 1897–1914*. (Modern British History.) New York: Garland. 1982. Pp. 253. \$50.00.

British suffragist historiography is divided, like Gaul, into three parts. There are first the comprehensive general histories and biographies produced by the suffragist generations (Blackburn, Strachey, Fawcett, Sylvia Pankhurst, and others), whose commitment did not preclude a fairmindedness that was remarkable in the circumstances. Second, there is the sequence of academic monographs (Morgan, Rover, Rosen, and others), which has added so much perspective and well-documented detail. Third, there are the celebratory feminist studies published during the 1970s (Rowbotham, McKenzie, Raeburn, and others), which often betray the scholarly standards set by their feminist predecessors. Fortunately, this book belongs to the second category.

By documenting the nonmilitants' central and pioneering role in reviving suffragism during the late 1890s and extending its class base, Leslie Parker Hume rightly locates the militant suffragettes in a subordinate role. She amply demonstrates the militants' "disastrous repercussions" (p. 137) on the women's suffrage cause, and especially on the fate of the nonparty Conciliation Bill between 1910 and 1912. Concentrating primarily on parliamentary events, she does not elaborate on the mood, inspiration, and structure of the extraparlimentary movement, whose indirect and long-term influence perhaps did as much for women as the reform it promoted. But she portrays the National Union of Women's Suffrage Societies (NUWSS) as energetically moving into new areas after 1912 by widening its impact on public opinion and strengthening its working-class links. Her clearly written, well-documented, and accurate narrative rests on a wide range of relevant sources—annual reports, periodicals, and manuscript collections. It does not greatly alter our overall picture of the Edwardian suffrage scene, but it does provide chapter and verse for an important and neglected aspect of it.

By truncating her account of the nonmilitants at both ends, Hume rather understates their achievement. She begins with the formation of the NUWSS in 1897, rather than with the nonmilitants' origins in the 1860s. She is therefore unable fully to demonstrate their major achievement: getting the intellectual case for women's suffrage on its legs. And she is surely wrong to see the militants as "the philosophers" of the women's movement (p. 191), if only because the militants were so distracted by nonintellectual considerations. Furthermore, on her own evidence, it was the NUWSS rather than the militants who "gave life" (p. 191) to the Edwardian suffrage movement; nonmilitant vitality in Lanca-

shire from the 1890s even gave birth to the militants' organization, the Women's Social and Political Union. Hume's decision to stop at 1914 prevents her from demonstrating the nonmilitants' strategically important tact in accepting an illogical but inevitable compromise at the suffragists' political climacteric of 1916–17. Instead, she can do no more than emphasize their prewar mobilization of opinion as a factor that wartime politicians took into account (p. 225). Nor does her concluding section follow the nonmilitants through to the postwar feminist generation where they were so influential. Only one defect of the third category within British suffragist historiography is evident here: a mild dose of tunnel vision. Hume does not fully set the suffrage issue into its context by clarifying the rival preoccupations that filled the political arena. This omission is most serious when she gives such minimal coverage to the adult suffrage route to women's suffrage. This was rejected by militant and nonmilitant suffragists alike, but was the only way of getting women's suffrage from a genuinely progressive Liberal government. Hume also exaggerates the extent of the nonmilitants' break with liberalism and the Liberal party after 1912; the Labour party connections of the NUWSS were always internally controversial, and were eventually quietly dropped in the changed party political situation created by wartime coalition.

It would be churlish for a British historian to grumble at an American publisher because this valuable book is reproduced from typescript, or because its index is unduly preoccupied with proper nouns and insufficiently differentiated in its page references. Instead he should be grateful that in these difficult times it has been published at all.

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DIANA GITTINS. *Fair Sex: Family Size and Structure in Britain, 1900–39*. New York: St. Martin's Press. 1982. Pp. 240. \$27.50.

Diana Gittins's aim in *Fair Sex: Family Size and Structure in Britain, 1900–39* is "to analyse the dramatic decline in family size which occurred among the working classes in England and Wales between 1900 and 1939." She qualifies this statement by asking readers to bear in mind that her book "is very much an exploratory study." It is intended to interest "sociologists, social historians, demographers and those involved in women's studies" (pp. 9–10). In chapter 1 she considers a number of theories about the decline of the birth rate, not only in her own period but from the works of Malthus in the early nineteenth century onward. Her analysis starts in chapter 2 and occupies the rest of the book.

The work resulted from an MA thesis at the University of Essex. Although much has been added and it contains some interesting information, it bears an unfortunate resemblance to such a piece of student research. In chapter 1 she might well have been trying to prove to her tutor that she had considered the most important, approved authors on factors affecting the birth rate—she mentions fourteen of them by name and an even larger number of theories in twenty-two pages. Thus, she has less space for the book proper, which starts with chapter 2.

Gittins merely touches on important topics such as the hostility of older women to birth control and the growth of women's light industrial work (pp. 178, 182). She spends too long on points of information that are either self-evident or have already been well documented, for instance, the crucial effect of the age at which women marry as a determinant of family size (pp. 66–67, 71–94 and following). Although she stresses local and regional variations in patterns of family size, she appears to know very little about some of the regions she mentions, and she almost wholly omits the effect of religion on maintaining the patriarchal family. Her sample of twenty-nine in-depth interviews is too small to be convincing. She could have widened the scope of evidence by reading some of the autobiographies of women who lived through these times such as Jane Walsh's *Not Like This* and Margaret Bondfield's *A Life's Work*, which very frankly discusses the attitudes of shop assistants toward marriage as an assurance against destitution in old age and their dread of bearing children.

There are many surprising omissions of relevant material, for instance, regional and class variations in maternal mortality statistics and the economic recovery due to rearmament in the second half of the 1930s. Her sense of historical epochs is generally weak. Some of her statements are incorrect, for example, "residential service was less and less common and was being increasingly replaced by the 'daily'" (p. 76). In fact, the 1931 census for England and Wales shows an increase in absolute numbers over those for 1921. Far from being atomized by the failure of the 1926 General Strike (pp. 44–45), many working-class communities in heavy industrial areas were more close-knit. Presumably "the decline of trade union memberships" (p. 183) is that of the years 1921–34, for at other times during 1900–39 memberships were rising and women's membership finished the period higher than ever before.

Yet despite omissions and inaccuracies, Gittins focuses attention on the importance of women's status and attitudes in determining family size and may inspire further research.

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BARRY D. HUNT. *Sailor-Scholar: Admiral Sir Herbert Richmond, 1871–1946*, Waterloo, Canada: Wilfrid Laurier University Press; distributed by Humanities Press, Atlantic Highlands, N.J. 1982. Pp. xii, 259. \$13.75.

"He was impatient," said a Cambridge colleague about Admiral Sir Herbert Richmond, "of the quiddities and taradiddles." Not a bad epitaph. It is very much in accord with Barry D. Hunt's fresh assessment of a prickly, brilliant, often extremely prescient naval officer. A timely book, it deserves to be read by a wide audience.

Every navy needs its Richmond. Born into a family of artists, he was drawn as a boy to the great national institution of his day, the Royal Navy. Like all truly eminent warriors, once he had mastered the technicalities of his profession he rose easily into the higher ranks. It was, sadly, not a harmonious career. At every crucial rung of the ladder at its higher levels, he seemed not to be able to stop himself from outraging convention.

Not only his own profoundly conservative service took umbrage. Winston Churchill effectively sidelined the presumptuous captain who questioned the close blockade of Germany and the strategy chosen for the Dardanelles. His proposals for the offensive use of naval and air forces received Admiral Sir David Beatty's blessing but fell on stony ground at the Admiralty. Between the wars he gave highly regarded but for the most part unheeded strategic advice that he "lived to see . . . being hastily and desperately emphasised" (p. 148). It is clear that Richmond was not, as some have suggested, simply an *enfant terrible* putting crackpot ideas into the minds of the public; rather he was one of the few sane voices in the official wilderness. His grasp of Britain's naval past, unmatched by most historians, gave him a crystal-clear view of the nation's needs. The tragedy of it all, and this must be seen in the context of modern naval developments, is that "he failed to provoke a reexamination of official thinking" (p. 202). Instead, he earned a premature "voluntary retirement."

There was considerable recompense, and this not only from the distinguished academic career that followed. He had founded and nursed through its notable growing pains the Royal Navy's professional journal, the *Naval Review*. He had written some of the best accounts of British naval history in the literature. He had launched the Imperial Defence College by being its first president. He had played a significant part in asserting British policy on belligerent rights. His point of view was almost always borne out by events, particularly during the Second World War. There is much to learn from that. Ignore at your cost the voice in the wilderness. Bring on the intellectual mavericks.

An essential companion to the Richmond papers published by Arthur Marder, this book is not intended to replace the analysis of Richmond as historian in Donald Schurman's *The Education of a Navy. Sailor-Scholar* is marked by rigorous scholarship and a graceful style. The admiral who helped change the face of the Royal Navy by such methods would have approved.

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DENIS JUDD and PETER SLINN. *The Evolution of the Modern Commonwealth, 1902-80*. Foreword by SHRIDATH S. RAMPHAL. London: Macmillan; distributed by Humanities Press, Atlantic Highlands, N.J. 1982. Pp. xiii, 171. \$24.00.

Denis Judd and Peter Slinn have undertaken the commendable but daunting task of tracing the evolution of the sprawling, ramshackle British empire of the early twentieth century into the modern Commonwealth of forty-six nation-states that exists today. They begin with a synoptic glance at the strength and size of the empire in 1902 and at the prevailing attitudes and methods of its rulers, and then quickly summarize the shifting constitutional relationships between Britain and its various possessions over the next eighty years. Appropriate attention is given to the working out of the relationships with the white settlement colonies of Canada, Australia, New Zealand, and South Africa, so important to the overall development of the modern Commonwealth, and to the complexities of the relationship with India. The African colonies, those in the Middle East and Southeast Asia, and the possessions in the Caribbean and the South Pacific are dealt with more cursorily and, given the rapidity of change, more summarily.

The metropolitan viewpoint prevails, and only occasionally do the authors venture into the intricacies of domestic colonial circumstance. There is little attention given to an analysis of the economics of empire. Not only are the patterns of trade and investment not investigated in depth, but also the intertwined and bedeviling elements of color and class are not explored at any length. Undoubtedly, a longer book would have been required in order to undertake such tasks fully. As it stands the volume is not, therefore, fully rounded.

Its great strengths, however, are its lucidity and the care that the authors have taken to bring their study as close to the present as possible. While this does result in a somewhat breathless rush through the developments of the last twenty-five years, it does mean we have a reliable overview of the move

toward Zimbabwean independence and the impact of Britain's entry into the European Economic Community. Similarly, the assessment of the role that the Commonwealth can and does play in the contemporary world is a thoughtful and salutary one; a refreshing counterbalance to the usual dismissive remarks.

On balance the book will prove most useful to the audience at which it is aimed, the high-school and university student. It is a well-written, clear-headed book of value to the uninitiated. For discussions over interpretation and for the full-scale debates that rage within the fields of imperial and Commonwealth studies on a myriad of topics the reader will have to look elsewhere.

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IAN B. COWAN. *The Scottish Reformation: Church and Society in Sixteenth-Century Scotland*. New York: St. Martin's Press. 1982. Pp. x, 244. \$25.00.

In the postwar period the study of the history of Scotland, particularly in the sixteenth century, has been accorded unprecedented attention from scholars at home and abroad. According to Ian B. Cowan, few of those who have in the past twenty years written on the Scottish Reformation have remained unbiased, and most have concentrated on particular aspects of the upheaval. In general the spiritual impact on society has not, however, attracted much attention. Cowan's aims are, therefore, to rectify some of these defects and to present a "personal interpretation of the more recent views on the causation and emergence of the Reformation in Scotland" (p. ix).

Cowan examines religious practice, education, and poor relief; and then, after paying attention to the monks and the friars, the latter of whom he justly criticizes, he concludes that the impact of the pre-Reformation church on society was not so much in decline as has been previously maintained. Indeed he considers it an open question whether the church in the first half of the century was faltering in its purpose. He rightly points out that, while public appointments of laymen to the great benefices did not enhance respect for church dignitaries, the integrated social and religious life of the burghs, centering so much on the parish church, could have been a source of strength for the future. The best prospects for the church lay in the movement of Catholic reform as witnessed in the church councils of the late 1540s and 1550s. The reform program set out in their statutes and canons is linked exclusively to reforms being initiated at the early sessions of the Council of Trent, but perhaps this "Counter Reformation before the Reformation," as Gordon

Donaldson has earlier described it, should also be seen in its European context, including reforms that were being advocated little more than a decade previously in parts of the Holy Roman Empire, particularly in the archdioceses of Cologne and Mainz. Indeed this is the proper context in which to discuss Archbishop Hamilton's catechism, which Cowan, following Durkan, rightly ascribes to the Dominican, Richard Marshall, former Prior of Newcastle, at that time probably teaching in St. Mary's College. Although the will to reform among churchmen was not so strong as it might have been, the crown, he believes, could have accomplished it by breaking the vicious circle of political appointments to high office in the church and by making more spiritual preferments. In the end "secular attitudes which had been bred within the Church coupled with even stronger manifestations of secularism outside its ranks proved too strong for the survival of the existing system" (p. 88). It is one of the main themes of this work that those forces together with wider political developments, rather than the growth and strength of Protestantism, which in the 1540s and 1550s, even after Knox's visit, had very limited support, led to the overthrow of the church and the emergence of triumphant Protestantism. Indeed, had not political events dictated otherwise, "all might not have been lost" (p. 87). That the success of the Scottish Reformation depended on popular urban support must, it is contended, be questioned.

In the second half of the book much interesting material, the result of recent research to which Cowan, as the bibliography illustrates, has extensively contributed, is brought together. Particularly instructive are the discussions of the polity of the reformed church and the assessment of the fortunes of recusancy. The final chapter takes up a recurring but often neglected theme of the Scottish Reformation, the virtual absence of intolerance and the comparative generosity in the treatment of the former clergy and benefice holders of the medieval church.

This is on many counts an important book, which will undoubtedly serve a useful purpose for the specialist as well as the general reader. It will, however, raise doubts in the minds of some, who will be critical of the virtual absence of any substantial discussion of the role of political and ecclesiastical leaders. It is as if the author were engaged in painting in the details on a large canvas, while leaving the leading characters obscured, if indeed he thinks there are any. It may be true that Knox did not occupy so prominent a role as has sometimes been believed and that he would not have loomed so large in later accounts had not he himself left his own "history" of events, but any treatment of the church and society in Scotland that takes little or no

account of the personality and involvement of prominent figures—Beaton and Wishart, Knox and Queen Mary, Lethington and Lord James, Morton and Melville—will to some read like *Hamlet* without the prince.

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PETER D. ANDERSON. *Robert Stewart: Earl of Orkney, Lord of Shetland, 1533–1593*. Edinburgh: John Donald; distributed by Humanities Press, Atlantic Highlands, N.J. 1982. Pp. viii, 245. \$31.50.

The Orkney and Shetland Islands are peculiar places. Settled by Norsemen and possessing Norse institutions, they were transferred to Scottish sovereignty in the fifteenth century but have maintained some distinctiveness in spite of centuries of Scottish colonialism. Events in these islands were seldom central to either Scottish or Scandinavian history, and historians have tended to ignore them or relegate them to footnotes. Few but natives have been concerned to give an account of the islands' history, especially that of the early modern period, which gives few occasions for expressions of romance or nationalism.

Gordon Donaldson, in work published mostly in the 1950s, was a significant exception to such historical neglect; and his influence seems to be partly responsible for a recent flurry of research and publication about the area. Current books by Alexander Fenton, Frances J. Shaw, and William P. L. Thomson, for example, examine the islands' social and economic life. Peter D. Anderson has now contributed a study of what he sees to be the last political expression of the territory's semi-independence, the brief resurrection of "an ancient and moribund body politic" (p. 150) by the two Stewart earls of the late sixteenth and early seventeenth centuries. This is a study of Earl Robert, one on Earl Patrick is projected.

Anderson's book has two focuses. One, as the title suggests, is the life of Robert Stewart, bastard son of James V, a man described as "personally unpleasant . . . aggressive, oppressive and ambitious in directions intended only to satisfy his personal desires" (p. 144). The problem with reconstructing the life of this "unlovely" character, though such traits were far from rare in the sixteenth century, is that almost no personal records exist. Facts about his life are drawn mainly from official and legal sources or from comments by enemies. Much inevitably remains obscure, and there is no way to evaluate his ideas or attitudes. But this book probably contains all one can know about Earl Robert.

The Orkney Islands are the other focus, for there

is relatively little in this book about the Shetlands. Early chapters sketch in the fifteenth- and sixteenth-century background. What can be discovered of Earl Robert's activities is spelled out later. Developments in the islands, which are potentially very interesting in themselves and as a variation of Scottish life in the period, are, unfortunately, regularly subordinated to materials on the earl. It is, for example, impossible to get from this account any idea of either the religious or cultural circumstances of the time, presumably because Earl Robert participated actively in neither.

This is a work for scholars, and it reflects Anderson's long and thorough research. It is not so much the story of Earl Robert and the islands as it is a report on the sources studied. Only the last two chapters attempt to place all this in some perspective. The book contains an excellent glossary, though one key term ("udal") is not included. There are maps, extensive notes, a bibliography, and six appendixes on research problems. A reader often needs considerable prior knowledge of Scottish and Orcadian history to understand the text, so it is inappropriate as an introduction to the period. Nevertheless, the author has contributed substantially to our knowledge of a too often neglected time and place.

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WILLIE ORR. *Deer Forests, Landlords, and Crofters: The Western Highlands in Victorian and Edwardian Times*. Edinburgh: John Donald; distributed by Humanities Press, Atlantic Highlands, N.J. 1982. Pp. viii, 226. \$31.50.

Devotees of John Bateman's splendid guides to nineteenth-century British landownership have long pondered some Highland entries. Certainly, Bateman's last list (of 1883) was a great improvement on the official Scottish "Domesday" (*Parliamentary Papers* [1874] vol. 72, part 3). Yet some reports are astonishing. The Duke of Sutherland's 1,326,453 Scottish acres paid him only £71,939, while the Duke of Argyll's 175,114 acres were worth merely £50,842, and the Earl of Breadalbane's 438,358 yielded £58,292. And when one moves from the Whig grandees to less pretentious (though still broad-acred) lairds, there are equally strange records: the Duke of Atholl drew only £42,030 from 201,640 acres, Lord Lovat £30,300 from 181,791, Lord MacDonald £11,613 from 129,919, Baillie of Dochfour £11,881 from 92,648, Farquharson of Invercauld £12,974 from 109,561, the Macleod family £8,464 from 141,679, and Cluny Macpherson £4,500 from 42,000 . . . but the list is too long!

Most of us assume that such statistics are explica-

ble by the nature of the land—moor, mountain, bog, loch, forest, and uncultivable waste—remote, beautiful, unspoiled, and romantically attractive to potential tourists, but unprofitable for much farming enterprise unless heavily subsidized by central or local government or by their agencies. Of course, a radical Highland alternative viewpoint has long been expounded, curiously compounded of nostalgic Jacobitism, a worthy desire to preserve an ancient language and culture, a plethora of economic complaints and solutions, and, sometimes, a touch of abrasive and racist nationalism. Real and alleged offenses against unwritten mores long rankled, which fueled the Crofters' party of 1885 and the Scottish National party of 1934 alike.

Once a Highland shepherd, Willie Orr explains some of the reasons for the Highlands' long malaise. Economic (pejoratively capitalist) forces led from eighteenth-century cattle raising via sheep farming to deer preservation. Consequently, parts of Scotland (about three and a half million acres at the peak in 1912) became a sporting area, apparently to Orr's regret but surely to the benefit of the sparse, unappreciative indigenous population. An influx of wealthy sportsmen from England, Europe, and America—while providing jobs, trade, and other revenue (through often contested local rates)—offset some social susceptibilities.

Orr can point to some bitter confrontations between crofters and sportsmen, although he also notes that some estates made local expenditures of all profit. Lairds (sometimes misnamed) and sporting tenants (especially the insensitive W. L. Winans, the American engineer), however, remain his villains, even though the lairds (notably the maligned Lovat) did much to renew Highland vitality. Palpably, politics is not Orr's forte, and he should have avoided speculation on political and other matters for which he has no evidence.

Yet this is an interesting, pioneering book, well documented (especially on the creation of deer forests) and sensibly argued. If it retains signs of its dissertation origins, at least it bravely deals with *terra incognita*. Perhaps Orr's problem area will be saved by tourism and North Sea oil. For the moment a Scottish verdict applies—"not proven."

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KARL S. BOTTIGHEIMER. *Ireland and the Irish: A Short History*. New York: Columbia University Press. 1982. Pp. ix, 301. \$19.95.

This book tells the story of Ireland as well as it can be told, on the basis of up-to-date research, within the limits of three hundred pages. In the first seven chapters it provides a comprehensive account of

Irish history from the times before the Celts to the present day. Karl S. Bottigheimer then boldly adds two chapters, one on the Irish overseas and one on Irish literature.

Nothing vital is omitted, and it is only in concise accounts such as this that the larger problems of Irish history can be stated. Ireland appears as an island colonized by successive waves of immigrants: Celt, Viking, Norman, English, and Scots, acquiring a Celtic character toward the end of the second millennium and remaining Celtic in spirit in spite of subsequent settlements, religious divisions, and the loss of Gaelic in the nineteenth century. Having acquired linguistic links with England, the Irish became colonists themselves and formed communities wherever the English settled. At the same time, by writing in English, they contributed an Irish dimension to English literature, which in the twentieth century gives Irish history an international significance.

This volume will serve the needs of those of Irish background who wish to know more about their past and of all those in contact with Irish communities. Above all, it will be useful to those attracted to Irish literature. Apart from this, there are few specialists in Irish history who will not benefit by seeing their field of specialization put into context and who will not acquire knowledge outside their own fields of interest. Specialists will nevertheless feel that their own areas of interest have been neglected. For example, the Orangemen do not appear in the index or, as far as I can discover, in the text, in spite of the role they have played in Irish history, including that of the Irish overseas, during the last two centuries. Adequate space is allotted to the Fenians, but they might have been placed in context as the first Irish-American-inspired movement to play a part in Irish politics. The treatment of O'Connell is adequate, but Bottigheimer does not present the O'Connellite movement as an aspect of an alliance between Irish nationalism and British liberalism, which began with Grattan and was continued with interruptions by O'Connell, Cardinal Cullen, and Parnell, ending with Redmond in the twentieth century.

Bottigheimer is more at home in social and political matters than in the religious dimension of Irish history. This helps him to maintain the balance of sympathies when dealing with denominational strife. It nevertheless leads too easily to secular explanations of the religious aspects of Irish history. The Irish paid a high price for their faith, but without it they could hardly have survived as a cultural entity.

Bottigheimer's viewpoint appears to be that of a secular and socially minded liberal. Not surprisingly, this leads him to conclude on a note of cautious optimism with the suggestion that prosperity arising

out of participation in the Common Market might erase the conflicts of the past. There will be some dissent from this view, but there is not likely to be a better-balanced and better-written coverage of the substance of Irish history for many years.

HEREWARD SENIOR
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COLM LENNON. *Richard Stanihurst: The Dubliner, 1547–1618: A Biography with a Stanihurst Text on Ireland's Past*. Dublin: Irish Academic Press; distributed by Biblio Distribution Center, Totowa, N.J. 1981. Pp. 186. \$27.50.

This publication by Colm Lennon is welcome as the first full-scale study of an eminent Anglo-Irish polymath and *homme d'affaires* of the early modern period. The book is divided into three sections: (1) Stanihurst's biography, (2) a comparison of his two major writings on Ireland, published in English in 1577 and in Latin in 1584, and (3) Lennon's English translation of key passages from the Latin work, which adds to the value of the book by granting undergraduates access to important primary material.

Throughout his varied career Stanihurst thought of himself as a Dubliner. Lennon reveals this Dubliner's changing perceptions of his aspirations and public duties. The process of intellectual transformation in a leading group of Palesmen from an identification with, to a rejection of, English policies in Ireland has recently been the subject of a study by Brendan Bradshaw, *The Irish Constitutional Revolution of the Sixteenth Century* (1979). Lennon confirms and extends Bradshaw's findings, with qualifying nuances, in relation to an important member of this group. Conditioned by family tradition, the young Dubliner Stanihurst was deeply imbued with English concepts of law and order that governed the policy of reformation of state and society by assimilation to the English and Anglican model in the 1560s. This was linked with a decidedly antagonistic attitude toward the Gaelic Irish. In the 1580s, however, in voluntary exile abroad, the same Dubliner championed in print and through unofficial diplomatic intervention the idea of a common Catholic destiny of all Irishmen under Spanish protection. The Gaelic Irish were now deemed responsive to the civilizing influences of the preachers of the Counter Reformation.

Lennon's comparison of Stanihurst's two major historical works on Ireland—his contribution to Holinshed's *Chronicles* in 1577 and his *De rebus in Hibernia gestis libri quattuor*, written and published in exile in 1584—throws welcome light on the changing outlook. Stanihurst's stated purpose in writing the *De rebus* was to combat the prevalent ignorance

of Ireland and to enlist Spanish support for the Irish cause. He had clearly abandoned the official Tudor attitude to his native country that he had expressed in 1577. One cannot quarrel with Lennon's contention that this broadened vision was caused by the failure of English social and religious policy in Ireland, to which Stanihurst responded as a humanist historian. This argument could have been strengthened by a more thorough exploration of humanist historical method in the sixteenth century. Stanihurst's belief "in the power of history to teach lessons" (p. 106) accords with the general view of northern humanists, notably Erasmus and Melancthon, that such lessons were useful to reform the moral conduct of individuals in their community. Stanihurst's historical method was not original, he was remarkably adept at using established humanist historiographical conventions. It was because these conventions were so well established among European scholars that *De rebus* could appeal both to Stanihurst's educated compatriots and to the political intelligentsia in the Spanish Habsburg lands. It assisted the former in adjusting their political views. It encouraged the latter to develop a pro-Irish policy.

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NICHOLAS CANNY. *The Upstart Earl: A Study of the Social and Mental World of Richard Boyle, First Earl of Cork, 1566–1643*. New York: Cambridge University Press. 1982. Pp. xii, 211. \$34.50.

This is a book that fits none of the familiar categories. The preface warns that it was "neither conceived nor executed as a biography of Richard Boyle." The title suggests, if not a biography, then a monograph, but the contents reveal something more on the order of an interpretive essay. There are similarities with Alan MacFarlane's *The Family Life of Ralph Josselin*; but as one of the wealthiest subjects of the early Stuart realm and certainly the wealthiest of the New English planters in Ireland, Richard Boyle is a far less representative figure than the East Anglian village cleric Ralph Josselin.

Nicholas Canny's avowed purpose is the rehabilitation of Boyle's unsavory reputation as a predatory colonist, and he succeeds in exhibiting admirable, hitherto neglected, aspects of his character, particularly in the earl's mellowness of advanced age. But some will wonder whether the injustices done the first earl's reputation, both in his time and after, required a volume to set them right; and others will doubt that Canny has proved his case. He purposely neglects the entire process by which Boyle acquired

(some would say "stole") his Irish fortune, arguing that it was adequately treated in Terrence Ranger's 1958 Oxford doctoral thesis on the subject. Ranger's thesis, which is far less generous toward Boyle than Canny's, has never been published (short articles based on it appeared in *Irish Historical Studies*, 10 [1956–57] and *Past and Present*, 19 [1961]), and the crucial omission of Boyle's acquisitive career raises the suspicion of special pleading. No more could one hope to revise the reputation of J. P. Morgan by concentrating exclusively on the accomplishments of the Morgan Library! The author promises to take up these missing matters in a forthcoming volume on "Ireland in the English Colonial System, 1580–1650." Canny is a prolific, fertile historian from whom much is reasonably expected, but for the moment Boyle could stand some further rescuing before his statue is erected in Dublin on the stump of Nelson's Pillar.

The most interesting parts of *The Upstart Earl* attempt to use him to test recent hypotheses about early modern European society. A chapter on "The Family Life of Richard Boyle" shows that, although the first earl saw remarkably little of his seven sons and eight daughters during their childhoods, affectionate relationships emerged between father and children. This finding is contrary to what Lawrence Stone described as the norm for this period in *Family, Sex, and Marriage*. Equally arresting is the enthusiasm with which Boyle embraced aspects of the Irish culture and society that he and the other New English were supplanting. He patronized a Gaelic poet, Piers Ferriter, put his infant children out to wet nurse with Irish women, married some of his daughters to Old English (partly Gaelicized) families like the Barrys and Fitzgeralds, and was concerned to provide education for Irish children in the schools he founded and supported. Clearly his attitude toward Ireland and the Irish was more complex than has previously been realized.

Canny portrays a parvenu: tyrannical, socially insecure, constantly striving, and astute enough to see that he might derive from Ireland a status and dignity that England persistently denied him. Occasionally, Canny's enthusiasm for an idea overpowers his good sense, as when he cites a passage attributed to Sir John Temple as evidence that the New English were "being drawn toward Gaelic culture, with the result that the planter society, of which Richard Boyle was one of the outstanding members, was becoming part of a larger hybrid Anglo-Irish society" (p. 138). Most of what was written along such lines in seventeenth-century Ireland was written after the "massacre" of 1641 by refugee Protestants seeking to vilify their Catholic former neighbors with whom they had fancied themselves as having been on such excellent terms. Conquerors frequently misperceive the courtesy and cordiality

of the vanquished, and Sir John Temple here seems an example and therefore an unreliable witness.

If *The Upstart Earl* is *sui generis*, how is one to judge its success or failure? As a suggestive, imaginative essay it will undoubtedly stimulate additional thought and research, but it repeatedly strains to generalize about the New English, as a class, on the basis of a sample of one. Either a full study of Boyle or a monograph on planter society would have carried more conviction. *The Upstart Earl* falls somewhere in between.

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BARON HERVÉ PINOTEAU. *Vingt-cinq ans d'études dynastiques*. Paris: Christian. 1982. Pp. 594. fr. 460.

This collection of twenty-two studies published from 1954 to 1978 will be welcomed by many who have sought in vain to locate copies of the little-known and rare periodicals in which they originally appeared. Baron Hervé Pinoteau's three major themes here presented are genealogy, heraldry, and symbolism of state, particularly as they refer to the medieval Capetians and their modern descendants, the Bourbons, in whichever country they now live. Long years of research in libraries and archives have enabled Pinoteau to collect an impressive quantity of material, both textual and iconographical, that had escaped the attention of others working in the field. The study (pp. 100–40) of royal symbolism in the presentation copy of Jean du Tillet's *Recueil des roys de France* is quite well known, but of equal if not greater importance are the articles on the royal crowns, seals, and banners of the Middle Ages. For those with more recent interests, there are a dozen studies of the Napoleonic coronations and symbolism and of the nineteenth- and twentieth-century Bourbons; contemporary historians will find that the book is an important statement of the views of the royalists currently opposed to the pretensions of the count of Paris. While most of the articles are photographically reproduced, all are preceded by introductions that correct the originals in the light of more recent research.

The average reader will find himself in strange terrain in Pinoteau's ardent legitimist and ultracatholic world, but that aspect of the work may be overlooked. Much more troublesome is the fact that many—indeed most—of the studies are much too loosely organized, and some amount to little more than a series of notes. (Particularly bad is “Le dossier nobiliaire et héraldique des Bonaparte” [pp. 235–70]. The well-written “Sacre et couronnements napoléoniens” [pp. 271–94] is, on the other hand, a real pleasure to read.) While a number of the

articles have become outdated, that is due in part to the fact that Pinoteau's studies laid the foundations for further research. The introductory matter before each chapter is difficult to use, as is the whole work, which, had it been entirely recast, rewritten, and made more reflective, would have made a worthy *thèse*. To find a given topic or reference is almost impossible because there is neither index nor bibliography, both of which had to be deleted for financial reasons. Even so, the book costs a hefty sum that will probably deter many who should use it.

Finally, Pinoteau's assertions are sometimes questionable or even downright wrong. This reviewer finds his dating of, and remarks about, many of the medieval coronation *ordines* unacceptable, and the same may be true of the early medieval scholars who read “Les origines de la maison capétienne” (pp. 141–96). Despite all criticism, though, it would be a mistake for either the medieval or the modern scholar to attempt to treat any aspect of French kingship without going to the effort of mining this book for its often unpolished gems.

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J. K. J. THOMSON. *Clermont-de-Lodève, 1633–1789: Fluctuations in the Prosperity of a Languedocian Cloth-Making Town*. New York: Cambridge University Press. 1982. Pp. xii, 502. \$59.50.

As one who has previously labored in these vineyards (or, should I say, apple orchards), my first reaction to this book was that the *maires* and *secrétaires-généraux* of long since declined French textile towns ought to be grateful that there exists a plentiful supply of aspiring Anglo-American historians willing to write the history of their industries and municipalities. It would seem that not even the present crisis and its concomitant phenomena of reduced fellowships and fewer jobs have diminished our ardor. And I suppose we shall go on, so long as (1) universities continue to grant PhDs and (2) publishers continue to publish, although at \$59.50 a time, I am not quite sure who, other than acquisition librarians, is going to buy the books.

These *généralités* noted, let me say at once that J. K. J. Thomson's book is a good one, solidly researched and reasonably well written, a serious contribution to our knowledge of the French textile industry and of the preindustrial economy in general. Whether 460 pages of text were the minimum necessary to make his views known is another question, but as elephantiasis of the revised doctoral dissertation is a common enough disease, it would no doubt be churlish of me to press the point too far.

The book is a chronicle of the fortunes of the Clermont woolen industry from the middle of the seventeenth century to the Revolution. The cyclical pattern of industrial development that Thomson sees as a universal feature of the preindustrial economy is here amply demonstrated, with alternating periods of prosperity and decline from the 1660s to 1789. The picture thus developed is hardly new, but it serves to reinforce what have become, over the last fifty years, common assumptions about the economic history of the period. By saying this, I do not wish to be unkind to the author, for I doubt that he had any other intent than to do just that.

The plot outline as it were, the history of production and sales of cloth, is but the background to a set of questions of greater importance concerning the influence of social relations on economic development. The growth of Clermont's industry was based on the exploitation of the Levant trade, and it is clear that burgeoning demand in the Ottoman empire was an absolutely essential element in creating opportunities for the cloth manufacturers of Languedoc. But the existence of demand does not guarantee that it will be fulfilled. As Thomson notes, industrial response to similar cost and demand situations will not always be the same: "The supply side is clearly relevant" (p. 17). (Perhaps another term might have been chosen, if only to avoid guilt by association; but Thomson is English and may not feel concerned by our parochial quarrels.)

Thomson bases his analysis on the interaction of an elite of clothing manufacturers and the French state. Arguing that the low status of entrepreneurship in a society where *dérogance* was still a key concept and that the continuing temptation to "disinvestment" in industry in favor of investment in other forms of wealth, notably land and offices, does not provide an adequate explanation of the slowness of French economic growth in this period, he finds that the Colbertist policy of administration, with its tendency to concentrate wealth in a few hands and prevent social mobility in the industry, along with its general inflexibility, bears a great share of the responsibility. The thesis is well argued and convincing, the more so as Thomson recognizes equally the advantages of Colbertist policy in favoring at its inception the expansion of the industry through the creation of royal manufactures and the establishment of structures that allowed the clothiers to develop their skills and thus to meet the demands of the new market. A policy that was advantageous in its early days could, and did, turn into its opposite as time went on. In the long run, the very entrepreneurial qualities it had encouraged were increasingly lost. Restrictive practices by both the state and the clothiers' guild, the limitation on apprenticeships, the refusal to admit new masters other than sons of

the old, and the increased control of the labor force all contributed to the decline of the industry. The clothiers who had risen to property and power at the end of the seventeenth century tended to lose the skills that had made them successful. They had become managers rather than entrepreneurs, resting on their laurels and no longer seeking innovative ways of meeting new market conditions.

The negative consequences of limiting production to a single type of cloth for a single market were increasingly felt. The important change in the demand situation of the 1750s, aggravated by the outbreak of the Seven Years' War, only made things worse. The manufacturing elite sought its way out of crisis by evading the regulations; and, when a change in administrative policy toward deregulation came about in the same decade, it was too late to stem the tide of decline. The elite manufacturers deserted the industry, either because they went bankrupt or because the profits still to be made, in the double context of the other outlets for their capital and the values of a society that disdained industrial activity, were no longer so attractive. They were replaced by poorer clothiers, some of whom still possessed the very artisanal qualities of skill and flexibility that had made their predecessors successful but who did not possess capital sufficient to allow for continuous production of high-quality cloth. Clermont textiles declined in quality, and the largely national market for cheaper cloth was not great enough to sustain the industry. The results were bankruptcies, unemployment, lessened social mobility, and the concomitant polarization of society into classes of propertyless wage-earners, struggling "middle-class" clothiers, and an older elite, that is, a rentier *bourgeoisie d'ancien régime*, striving to imitate the nobility by living idly and rigidly observing an increasingly anachronistic code of behavior.

Now that the argument has been presented (and the evidence suggests that it is sound), we may ask: Was all this, as Sellars and Yeatman would say, a good or a bad thing, advantageous or disadvantageous for later generations of Frenchmen? To which the only answer possible at the moment—and I am sure Thomson would agree—seems to be something on the Norin model: *Peut-être ben que oui, peut-être ben que non*. We all know—or knew until the recent onslaught by the unholy alliance of the Club of Rome and the "small is beautiful" enthusiasts—that economic growth is a good thing. But what if economic growth in one period reinforces traditional structures (or, rather, relationships) and those who by them exercise power? Those very same people may, in the next period, want to minimize further growth, especially if it implies not only quantitative, but qualitative socioeconomic change. Therein lies the real question: What is the relationship between quantitative and qualitative

change? It will not do to dismiss, with Crouzet, the notion of capital accumulation as a precursor of the industrial revolution, even when a good portion of that capital can be shown to have been, initially, hoarded or employed in less than optimally productive ways. The last pages of this book provide a good summary of state-of-the-art hypotheses on this and similar matters concerning continuities and disruptions between preindustrial and industrial-revolution growth. If Thomson makes no real attempt to resolve the issues—that would involve carrying his study further in time—he has the important merit of having drawn our attention to the interwoven complexities of industrial development and class structure.

One final word: yes, Virginia, class does exist, and what Thomson has shown us here is the rise and transformation of a bourgeoisie, an itinerary that made entrepreneurs of artisans and then turned them into rentiers, while the industry they had fostered fell into the grasp of a new group of manufacturers. That this bourgeoisie was also an elite in the proper sociological sense of the term does not make the assertion any less true, nor, *pace* Max Weber, does its adherence to an older set of values that placed emphasis more on "consumption patterns" than production mean that the society of which it was a part now reverted to social stratification based on orders and status. No matter what one may think of Roland Mousnier's thesis of a *société des ordres* as applied to earlier centuries, it is clear that class, in the simple sense of social relationships created in the process of production, had come to stay in France at the end of the old regime. The reassertion of older values and lifestyles by a given social group may be a sign of its own disarray, of its inability or lack of desire to make a clean break with ancient institutions; I do not think it can be made to signify a reversal of tendencies in historical development.

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MARCEL LACHIVER. *Vin, vigne, et vignerons en région parisienne du XVII^e au XIX^e siècle*. Pontoise: Société Historique et Archéologique de Pontoise, du Val d'Oise et du Vexin. 1982. Pp. 957. 350 fr.

This book falls in the tradition of the magisterial French *thèse*. Like Pierre Goubert's *Beauvais et le Beauvaisis* and Emmanuel Le Roy Ladurie's *Paysans de Languedoc*, it is the product of years of labor by one of France's great scholars, Marcel Lachiver, the author of several books on the countryside near Paris, notably the classic *La population de Meulan du XVII^e au XIX^e siècle*.

If Balzac wanted to redo the *état civil* in his *Comédie humaine*, then Lachiver has written a "Rise and Fall of Winegrowing" through his solid and elegant use of demographic and other statistical material. His goal was to "show how a market-oriented agriculture developed, how it triumphed, how it died from its very success" (p. 720). He also endeavored to "assess the special place the winegrower held in rural society . . . to see whether this winegrower, landowner, and individualist . . . behaved differently from other social groups" (p. 13).

The picture of the winegrower that emerges is one of a man who adapted so well to changing situations that he eventually participated in his own phasing-out. Literate, unusually endogamous, overwhelmingly given to contraceptive practices after 1800, winegrowers conserved and maximized what they had until their good business sense told them to switch to more profitable crops. The winegrower we meet in these pages is a very modern man, responsive to market changes and constantly willing to innovate.

This book is divided into four sections. In the first, which deals with the process of winemaking, we learn that the Île-de-France produced, until around 1750, a high-quality wine. But after the mid-eighteenth century, the winegrowers' competitive position in the market declined for various reasons. By the 1920s winegrowing in the Paris area had been replaced by other varieties of specialty farming.

Sections two and three analyze the economics of winegrowing and the social relations among winegrowers. They did not vary greatly in wealth, they gloried in their title of winegrower, and they passed their property on to their children before they died, enabling a new generation of winegrowers to form. To an astonishing degree, they married each other, often distant cousins from the same village, reinforcing community unity and creating veritable wine-growing dynasties.

But it is section four on demography that is especially outstanding. Although not the first to practice birth control in the Île-de-France, winegrowers, after 1800, took it up with a vengeance. Whereas, as Lachiver observes, in earlier centuries people had tried to adjust production to population size, the winegrowers of the nineteenth century tried to adjust population size to production. Lachiver does not suggest that it was only economic factors that moved the winegrowers to have drastically smaller families; he points to the importance of religious, cultural, and even unconscious factors in this adjustment of values and attitudes.

This book raises important questions about the nature of human adaptation. It does so in the best tradition of French scholarship, which, by taking local issues very seriously, manages to probe more

general historical and social problems with elegance and grace.

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ORVILLE T. MURPHY. *Charles Gravier, Comte de Vergennes: French Diplomacy in the Age of Revolution, 1719–1787*. Albany: State University of New York Press. 1982. Pp. xi, 607. Cloth \$49.00, paper \$16.95.

Several books have been written about Charles Gravier, Comte de Vergennes; but they are all limited to certain periods of his life, and they are all old, out of print, and generally inaccessible. This new biography by Orville T. Murphy is the first to cover the entire career of Vergennes, which extended from 1739, when he was sent to Lisbon as an apprentice diplomat, until his death in 1787.

Vergennes's first important diplomatic assignment was in Constantinople, where he was France's ambassador from 1755 until 1768. There he was "on the firing line" created by the Diplomatic Revolution. Murphy agrees with L. Jay Oliva that the French tie with Austria and Russia was a "misalliance" because it left unprotected France's traditional allies in Eastern Europe. Vergennes saw that the Ottoman empire was incapable of defending itself against Russian expansion to the Black Sea. The "Eastern Question" was born, and it brought in its wake the first partition of Poland. Farther north, in Sweden, the Russian presence was felt by the support given to the "Caps," the aristocratic party that had controlled the country since 1718. Vergennes was sent to Stockholm in 1768 and four years later helped Gustavus III overthrow the Caps. Two years later, in 1774, partly as a result of laurels won in that affair, Vergennes returned to Versailles to become foreign minister.

The advent of Louis XVI, young, inexperienced, but sincerely desirous of repairing the failures of the previous reign, enabled Vergennes to lay down certain principles of foreign policy. France should exert its influence to maintain the balance of power, prevent any further aggrandizement by the great powers at the expense of the small nations, and act as "honest broker" in international disputes in order to preserve the peace. France was well suited to perform this role because it had no territorial ambitions of its own.

How well did Vergennes carry out those policies? Murphy finds that his overall diplomatic achievement was "an ambiguous legacy." On one hand, by his astute diplomatic skill the "master juggler" did preserve the peace in several instances. He prevented a Spanish assault on Portugal and restrained the ambitious Emperor Joseph II, both in the Bavarian

succession crisis and on the Scheldt River. In the crisis of 1783 he failed to prevent the annexation of the Crimea and the Kuban by Russia, but he did succeed in limiting the losses of the Ottoman empire by preventing Joseph from joining with Catherine II in a partition of that empire.

On the other hand, Murphy asserts that Vergennes departed from those admirable policies in one important affair, and this ultimately undermined all his positive achievements. This was his ill-advised intervention in the American Revolution, which Murphy considers an unprovoked aggression against the British empire. He follows Samuel F. Bemis in believing that Louis XVI should have followed Turgot's advice to remain out of the war, rather than Vergennes's. "From the perspective of history, Turgot's option seems clearly the wisest," writes Murphy, because he "appreciated the seriousness of the financial situation" (p. 254). The ultimate consequence of the American War for France was, in Murphy's view, the beheading of the king whom Vergennes had so loyally served and the destruction of the social order to which he belonged.

Such a severe judgment of Vergennes's American policy is dubious. It may be compared with that of the Duc de Castries, who wrote in *La France et l'indépendance américaine* (1975) that France would have been richer had it stayed out of the war but would have been unable to defend itself from the British hegemony that would certainly have resulted from the crushing of the rebellion. It was the familiar dilemma of guns versus butter. If one assumes (as Turgot did) that the independence of the colonies was inevitable, then Vergennes's American policy was quite consistent with the aims he laid down at the beginning of his ministry.

That there was a direct connection between the financial costs of the American War and the violence of the Year Two is a tenuous thesis at best. In his biography of Turgot published in 1961, Edgar Faure found that the budgetary situation faced by Turgot was not all that dramatic. Dupont de Nemours, in his biography of Turgot published in 1809, denied that the deficit could have been the cause of the Revolution. None of the finance ministers who succeeded Turgot up to the Revolution believed that the finances were in such desperate shape as portrayed by Murphy.

Throughout this book one detects a certain lack of sympathy for the subject. Consequently the personality that dwelt behind the familiar portrait appearing on the cover remains elusive.

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WILLIAM COLEMAN. *Death is a Social Disease: Public Health and Political Economy in Early Industrial France*.

(Wisconsin Publications in the History of Science and Medicine, number 1.) Madison: University of Wisconsin Press. 1982. Pp. xxi, 322. \$35.00.

The subject of William Coleman's book is identified in its subtitle, "Public Health and Political Economy in Early Industrial France." More precisely, Coleman argues that the creative school of public-hygiene investigators active in Paris between the 1820s and the 1840s worked within the theoretical framework of liberal political economy. Through a close review of the published *oeuvre* of Louis-René Villermé (1782–1863), a leading figure in the French hygiene movement, Coleman explores the tensions between the hygienists' gloomy empirical findings and the economists' optimistic, laissez-faire theory.

Coleman's book is in three parts. Part 1 provides background in three areas of specialization for the general reader. One chapter is devoted to the development during the Revolutionary period of the Parisian school of medicine, from which Villermé and most of the hygienists came. Another chapter sketches the demographic, economic, and social conditions of the period, particularly the rapid growth of cities, which was the hygienists' subject matter. A third chapter provides a summary introduction to the major works of the early French political economists, notably Jean-Baptiste Say.

Part 2 examines all of Villermé's major contributions to public hygiene, from his study of prisons (1820) through the two-volume survey of the physical and moral condition of textile workers (1840). Until the late 1830s, when he began his study of the textile workers, Villermé was primarily a statistical investigator. His basic method was to discover the causes of ill health by comparing mortality rates for populations in different social circumstances. What he found in his studies of prisons, cholera mortality, and general mortality was the very strong relationship between economic condition and mortality rates. In all circumstances and at all ages the poor died more often than those who were better off. While Villermé continued to apply statistical analysis in his survey of the textile workers, his last major work was also the first in which he relied on the kind of on-site inspection and personal interviews that characterized the work of Jean-Baptiste Parent-Duchâtelet, the other major figure of early nineteenth-century French hygiene.

Part 3 of Coleman's book contains the heart of the argument on the relationship between political economy and public hygiene. The starting point for this argument is the collection of Villermé's lesser known polemical statements from the era of the 1848 Revolution, including a treatise on workers' associations—his contribution to General Cavaignac's restoration of order. These works show

that the public hygienists, having discovered so many social diseases, resisted any significant therapeutic intervention.

Coleman's intellectual explanation for the hygienists' attitudes is not fully satisfying. Other leading hygienists, introduced as an afterthought in the concluding chapter, did not necessarily share Villermé's politics. Many Catholic scions of Old Regime nobility, including Parent-Duchâtelet, Villeneuve-Bargemont, and Benoiston de Châteauneuf, were drawn to careers in public hygiene and public assistance in early nineteenth-century France. A few hygienists, Philippe Buchez and François-Vincent Raspail, sided with the social revolutionaries in 1848. Even the case of Villermé might profit from a more thorough intellectual biography, one which got into the hygienist's personal development and manuscript sources to resolve such questions as why he abandoned serious hygienic studies in 1840 and only began to speak out on political issues thereafter.

As it stands, Coleman's book raises important questions and provides the best available introduction to Villermé's work within the context of contemporary social theory and social conditions.

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C. JAMES HAUG. *Leisure and Urbanism in Nineteenth-Century Nice*. Lawrence: Regents Press of Kansas. 1982. Pp. xviii, 167. \$19.95.

This analysis of the tourist trade as a primary force controlling the development of Nice makes a significant contribution to the historical literature of urbanism. Important communities shaped mainly by tourism are not numerous, so that this study by C. James Haug of one of the world's greatest pleasure centers is of considerable interest, not only as a regional study, but as a description of a special type of urban growth process. The evolution of Nice from a haven for aristocratic European invalids to a Mecca for wealthy pleasure seekers of the late nineteenth century is a central theme.

The book explores the effects of the changing demands of the tourist trade on urban planning and decision making, particularly with regard to street plans, recreational facilities, and provisions for health, water, and sanitation. It also shows the impact of development on local residents. Of special interest is the author's study of census records, on the basis of which he describes the progressive growth of class segregation in residential districts and the degeneration of the old town into quarters for day laborers and Italian migrant workers.

Haug's chapter on the effect of economic problems on Nice's expansion explores in some depth

the forces that control resort growth. The vulnerability of an urban economy based on tourism to European political and economic instability is ably illustrated. Unfortunately, though he continues several of his chapters to 1914, the author does not extend his discussion of this subject beyond the effects of the depression of the 1880s. It appears that a considerable resurgence must have occurred before World War I, for population statistics indicate a rapid growth rate in the three decades following 1881. Continuation of the economic study through this period would have enhanced the work.

The author's interest appears to center largely in groups, trends, and movements. Most of his generalizations on these subjects seem well founded, but some give rise to questions. For instance, Haug repeatedly writes about the intentions and motivations of the municipal council, often in very critical terms, but he says little concerning the backgrounds and socioeconomic ties of the individuals who made up that council over the years. A more thorough analysis of the group responsible for formulating policy would lend greater authority to his judgments.

Although *Leisure and Urbanism* is not a definitive study, it provides an excellent introduction of interest both to social historians and to urban analysts. In addition to being informative, it is well written and entertaining to read, with useful charts, graphs, and illustrations. Finally, the work is based on thorough research in the French national, departmental, and municipal archives, augmented by extensive study of published materials relating to Nice, to tourism, and to economic and political backgrounds. For those interested in Nice, in the history of tourism, or in the role of the tourist trade in urban development, this book should prove valuable and stimulating.

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CLAUDE NICOLET. *L'idée républicaine en France, 1789–1924: Essai d'histoire critique*. (Bibliothèque des Histoires.) Paris: Gallimard. 1982. Pp. 512. 138 fr.

As Claude Nicolet notes, France is a republic, but it has at times been two kinds of monarchy as well as an empire and a caesarism. Moreover, it has had five different republics by official count—actually more, since there were really three republics (girondist, montagnard, and thermidorean) during the French Revolution, though these were counted as only one in the official enumeration. The original meaning of the term was an equilibrium between king and parliament. Later this changed to an assembly-dominated regime without a monarch or even without a strong executive. The presidency of

the Third Republic became a well-known joke. To add to the confusion, one of the monarchies claimed to be a republic; and the caesars, notably Generals Boulanger and De Gaulle, sometimes claimed to be more republican than the republicans. During the Third Republic it was fashionable to identify subrepublics: the republic of dukes, of pals, and so forth. Conservatives, liberals, radicals, socialists, radical-socialists, and even communists have supported the republic.

What is one to make of all this? The term "republic" like the related term "democracy" may be thought to have become so encumbered with diverse meanings, deposited by successive layers of history, as to be operationally useless today. We can describe its checkered history; but can we identify a "republican idea"? Nicolet, most of whose previous expertise lies in the area of the original republic of ancient Rome, is willing to make the attempt. It is not clear that he has surmounted some imposing obstacles. He tries to combine analysis with descriptive accounts of all the intellectual creations with which republicanism interacted, from Rousseau to Alain, including science, social science, and major philosophical positions such as positivism and Bergsonism. Relations between philosophical ideas and politics interest Nicolet, and he finds some interesting connections: Jules Ferry's debt to Auguste Comte, for example, or the impact of science on political principles in the Darwinian era. Forgotten amateurs functioning at the frontier where political theory and polemics meet, such as Célestin Bouglé, Alfred Fouillée, and Leon Bourgeois, engage his attention. The most interesting era is that extending from the early to middle Third Republic, when republicanism, at first a radical, revolutionary idea, lost its innocence, grew "opportunistic," was engulfed by the bourgeoisie, and even rallied to by the pious and the conservative, thereby becoming suspect to some on the left. Yet, when assailed by an antirepublican right, be it neomonarchist or fascist, the republic recovered defenders on the left. This rather intricate dialectic is enriched or confused by the whole ferment of modernisms in the era of Nietzsche, Sorel, the Second International, Durkheim, and the sociologists. Those who never tire of reading about this most fascinating of all periods of intellectual history will find some new matter here, along with a good deal of the familiar, in a volume that is far from concise. (It is lamentable that there is no index.)

In his search for an elusive "republican idea" Nicolet is inclined to exaggerate its role. Did the republican ideology underlie its success, as the author says (p. 269)? He claims its appeal is responsible for even the Communists' "rejoining the nation" in recent years; but surely this was for other reasons, such as repulsion from Stalinism and an evident

need for a new political strategy. It is by no means clear, to say the least, that the French Communists are genuine converts to democratic pluralism. In the end the success of a republican or democratic order must surely be put on precisely the grounds of its having *no* ideology, of being that which divides us least, and of resting on skepticism and pluralism—the worst form of government except all the others, which prove to be impossible. Although France is above all others the nation where ideas are important, in arguing that the republican regime is legitimized by an intellectual creed, the author of this imposing study, which draws on a wide array of sources both published and archival (though almost entirely ignoring the considerable scholarship outside the French language), may have fallen prey to an intellectualist fallacy. But this does not seriously detract from its value in presenting such a case with much learning and perceptiveness.

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ROBERT FRANKENSTEIN. *Le prix du réarmement français, 1935–1939*. (Série "France XIX^e–XX^e Siècles," number 13.) Paris: Publications de la Sorbonne, 1982. Pp. 382. 150 fr.

The lineaments of the French rearmament effort of the 1930s have been known ever since Edouard Daladier, ably assisted by Léon Blum, turned the tables on his Vichy tormentors and shut down the Riom "show" trail in late March 1942. On the basis of their testimony and of other papers that found their way into the public domain, several short studies of rearmament have since appeared, the best of which is still Jean-Marie d'Hoop's 1954 article in the *Revue d'histoire de la 2^e. guerre mondiale*. But relatively little attention has been paid to the cost and the financing of what is now considered to be the world's most massive rearmament in depth until Britain's industrial plant swung into high gear during the summer of 1940. Robert Frankenstein amply fills this gap with this study, excerpted from his 1976 article in the *Revue d'histoire de la 2^e. guerre mondiale* and his more recent doctoral thesis for the Sorbonne.

Frankenstein divides his book into three sections, the first concerning the cost, the second the financing of the four distinct phases of rearmament from the beginning of 1924 through the end of 1939. The third and final section is devoted to the role of public finance in the rearmament industries. Rearmament was extraordinarily expensive, close to two hundred billion francs at the 1982 level. It was financed not by taxes but by reckless borrowing at home and abroad, an unabashed *politique de facilité*,

as Pierre Mendès-France once put it. The French state, through its intervention in the rearmament industries of the 1930s, laid the bases for its present-day paramount role in the economy.

Displaying the mastery of a virtuoso over his complex technical data, Frankenstein never loses sight of the political, social, and ideological realities that encompass his topic. He shows clearly how the French possessing classes of the 1930s remained faithful to their old Orléanist precept equating war with domestic upheaval, violated only once in modern history when the heady *réveil national* of 1911–14 ended in the *année terrible* of 1917, with Lenin in Petrograd and mass strikes and armed mutiny at home. No more than the Nazi hierarchs did they consider rearmament as a solution to the terrifying social and economic problems of the Great Depression: it was viewed solely as a political mechanism to thwart Hitler's ambition to conquer Europe. The most flagrant example of their irresponsibility as a *classe dirigeante* was certainly the huge expatriation of capital during the Popular Front era: turned into psychoses by the Spanish Civil War, bourgeois nightmares of a general European conflict with Stalin as an ally and a Communist-supported government in Paris effectively overpowered the capitalist logic of higher profits in domestic investment, as Frankenstein wryly points out.

Although a fragile consensus emerged in 1939, with Paul Reynaud achieving a temporary victory over Pierre-Étienne Flandin, it was undermined first by a passionate national determination to avoid "real" war (bloodless economic weapons only—such was the lamentable fate of the nation-in-arms idea), second by social disintegration triggered by rearmament and mobilization (an alienated peasantry and proletariat, resentful of the "blood tax" and harsh factory discipline), and finally by the presence of the *Erlkönig* of the defeat himself, nestling in the center of Reynaud's flock of protégés, the sinister Yves Bouthillier. The way was now clear for the eventual seizure of "civil," that is political as well as economic, domination by the post-1945 structural *planificateurs* over French business, already conditioned for centralized control after three years as a cogwheel in Speer's and Goebbels's *totaler Krieg* machine—a domination rapidly "digested" by contemporary French neocapitalism, as the author concludes.

Frankenstein's study, which comes equipped with the necessary tables, graphs, and charts, is based on all of the relevant archives, with the major exception of the still-secret complete Riom instruction, which I used recently for a forthcoming study of Flandin and Daladier. (The chapter on rearmament done in the 1979 study of Riom by Henri Michel's staff gives little idea of the fantastic richness of this archive.) Nevertheless, on the basis of its erudition, clarity,

and originality, Frankenstein's book is of great value to both specialists in the 1930s and generalists in modern French and European history. It is not likely to be surpassed for many years to come.

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JOSÉ ANTONIO MARAVALL. *Utopía y reformismo en la España de los Austrias*. Madrid: Siglo Veintiuno. 1982. Pp. 398.

For the past forty years José Antonio Maravall has been a distinguished student in the field of Spanish history of ideas. His numerous works remain still our most important source and guide to the understanding of Spanish political thought in the early modern period. It is therefore disappointing to record that his *Utopía y reformismo en la España de los Austrias* (a splendid subject for a monograph) is nothing more than a compilation of articles (brought up to date, according to the author's introduction) on utopian and reformist thought in early modern Spain published at various times between 1949 and 1979.

In the introduction Maravall claims that the origins of Western utopian thought rest on the twin foundations of the Renaissance and the discovery of America. He believes that the novelty of the latter event and the exotic vistas that it placed before European eyes together provided an ideal field of action where the sociopolitical imagination of the Renaissance, thirsting for reform but fettered by the Old World's rigid institutional structures, could roam freely.

Immediately following some general reflections on "Utopian Thought and the Dynamism of European History" come the collection's two utopian essays. In the first, Maravall surveys the ideology of those Franciscans who, inspired by More's *Utopia*, attempted to transplant to the soil of New Spain the spiritualist currents of the late Middle Ages. They concluded that the two "nations" (Spaniards and Indians) coexisting in the New World were incompatible and should remain separate and insisted that the governance of the Indians be entrusted to them. The second paper suggests that the famous Bartolomé de las Casas used the myth of the New World, invented by Renaissance utopians, not as a norm to effect a reformation in the mores of the Old World but as the ideal to be given reality in America. Hence Las Casas's basic premise: the Indian polities possessed all the admirable traits of Utopia. Because outside influences threatened them, however, new social forms would have to be invented capable both of articulating indigenous and foreign mores and of reviving those native pristine qualities that had

already perished under the impact of foreign intrusion.

The two closing essays are devoted to the theme of social reform in sixteenth-century Spain. The first studies the thought of Juan de Robles, a Benedictine monk who, entering (1546) the polemic then raging between rival views on how to care for the poor, proposed a practical policy of social justice to replace the traditional reliance on mere charity. The subject of the second is Pedro de Valencia, critic of the judicial procedures followed by the inquisition in its dealings with witches, author of a plan to deal with the Moriscos, and thinker whose economic ideas Maravall sees rooted in his views as social reformer.

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MIGUEL ARTOLA. *La hacienda del Antiguo Régimen*. (Alianza Universidad Textos, number 42.) Madrid: Alianza Editorial Banco de España. 1982. Pp. iii, 511.

Analyzing the structure and evolution of royal finance from 1590, when its bureaucracy was in place, to 1808, when the system collapsed, Miguel Artola divides those two centuries into three long periods, each having its own characteristics, but all sharing a common dilemma. Time after time, sensible and necessary plans for fiscal reform foundered because of the strains of warfare and resistance from the economic elite.

From 1590 to 1665, although many taxes were frozen by the *Cortes* (parliament) of Castile, they continued to be paid with imposts on basic consumer goods. Faced with the expenses of the Thirty Years' War, the crown relied increasingly on taxes and monopolies created by regalian right, though the *Cortes* was able to redefine those taxes and take control of their collection, belying its spineless image. In the end, the old tax structure persisted, its deficits covered by loans, sales of jurisdictions and government bonds, and periodic *servicios* voted by the *Cortes* of Castile. In the eastern provinces of the Crown of Aragon, ancient charters held ordinary taxes so low that *servicios* were the main source of royal income, which helps to explain the volatile nature of eastern relations with the central government.

The period 1665–1779 saw several attempts at fiscal reform, beginning in the reign of the last Habsburg. Under the new Bourbon dynasty, administration was simplified and centralized, and the eastern provinces were brought into the system. The Council of Finance gained power as its hub, relegating the *Cortes* to a subordinate role. Royal

revenues rose dramatically in the eighteenth century. Yet the most serious attempt at structural reform, a plan to institute a single tax based roughly on wealth, was abandoned because of resistance from the secular and ecclesiastical elites.

Privilege and war finally brought the system to an irreversible crisis in the period 1779–1808. Failing to achieve the perennial goals of increased income without higher taxes and a more equitable distribution of the burden, the crown resorted to loans and other expedients to pay for wars related to the French Revolution and its aftermath. Neither the founding of a national bank, nor the issuance of more secure state bonds, could avoid ruin. Ironically, only war postponed the need to choose either repudiating the national debt or completely reorganizing royal finance.

Artola documents his story well, blending archival and secondary sources effectively. He is stronger on topics that he has studied himself than when he is forced to rely on the figures of other scholars in a field that is still in need of much rigorous archival work. His bibliography omits modern works, because they are mentioned in footnotes, but two indexes, one for proper names and one for subjects, can be used to fill the gap. Thirty pages of tables are also helpful. In all, this is a very useful book because it analyzes not only why an antiquated system eventually failed but also how and why it lasted for so long.

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REINHARD LIEHR. *Sozialgeschichte spanischer Adelskorporationen: die Maestranzas de Caballería, 1670–1808*. (Vierteljahrschrift für Sozial- und Wirtschaftsgeschichte, Beihefte, number 70.) Wiesbaden: Franz Steiner. 1981. Pp. x, 380. DM 74.

The *Maestranzas* were local associations of nobles founded with the aim, usually unfulfilled, of reviving horsebreeding and horsemanship by running studs and bullfights and organizing other public activities. They therefore enjoyed royal patronage and various honorific and judicial privileges. Almost all of them were established either in the reign of Charles II or in the years 1728–39, and they were (with the exception of Valencia), at this stage, an exclusively Andalusian phenomenon. Neither fact is sufficiently explained. Reinhard Liehr argues, from an analysis of the membership of the Seville *Maestranza* in 1670, 1737, and 1763, that the *Maestranzas* were not simply a noble reaction to the rising power of the third estate, as is usually asserted, but rather a means of integrating the newer, rich, immigrant noble families of the sixteenth and

seventeenth centuries with the old nobility resident in the city since the Middle Ages. Maybe, but there was not much social mobility about it. This was very much an association of equals in everything but seniority. They were landowners, holders of *mayorazgos*, members of the most prestigious fraternities, certainly involved, directly or indirectly, in the Indies trade, but none of them was in manufacturing, retailing, a craft, or even in any of the liberal professions, or exclusively in commerce or banking. The *Maestranzas* seem to have been a means of consolidating, one might even say freezing, the social mobility of the sixteenth and seventeenth centuries. They were also, though Liehr does not really investigate it, part of a process of political and social devolution, local and civilianized versions of the devalued Military Orders, validating blood (though with lower standards of proof), and secularized versions of the *sofradías*, publicizing wealth and status in ceremonial display.

It is unfortunate that Liehr's analysis had to stop in 1763, on the eve of major changes in the membership of the *Maestranzas* and at a time of rapid economic and social development. After a long period of lassitude from the 1730s, membership widened enormously after about 1765, annual admissions multiplied, as much as twelvefold in places, fewer were relatives of old members and more were foreign to the city. High entry fees were imposed, revenues increased greatly and so did expenditures. Their communal sessions declined, but their public appearances became more ostentatious and extravagant as did their contributions to civic welfare. Liehr's linking of these developments to the spread of agrarian wealth in Andalusia is not by itself entirely convincing. The history of the *Maestranzas* needs to be related to a more extensive analysis of social change in other municipal institutions than he undertakes and to changing attitudes to nobility in the Enlightenment, notably the new emphasis on personal wealth and community service manifested, for example, in the justifications of the *hidalguías* granted in these same years.

Liehr has devoted over three hundred pages of careful and competent scholarship, based on *Maestranza* archives in Seville, Granada, Ronda, and Valencia, and some four hundred items of bibliography, to a subject that in most books rates barely a sentence. He studies their establishment, their membership, their organization, their activities, their finances, their privileges, and their politics, yet, despite his efforts to present the *Maestranzas* as an important force for constitutional and social conservatism in the last years of the *ancien régime*, one cannot help feeling that his talents deserved a more significant subject.

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MICHAEL ALPERT. *La reforma militar de Azaña, 1931–1933*. (Estudios de Historia Contemporánea Siglo XXI) Madrid: Siglo Veintiuno. 1982. Pp. 338.

Although the history of the Second Spanish Republic (1931–36) has become a growth industry, the military reforms of Manuel Azaña, first Republican minister of war, have been neglected. This monograph is the first to describe the reforms in detail and to evaluate them as a solution to the military problems inherited from the parliamentary monarchy.

The purpose of this well-conceptualized study is to establish the origins of Azaña's ideas and to challenge the contention that he shaped his policy in response to "sectarian" pressures. In discussing the development of Azaña's views, Michael Alpert correctly points out that Azaña was consistently more interested in civil-military relations than in the so-called "technical" aspects of military organization. Alpert usefully provides a context for the reforms by discussing contemporary European military policy and earlier efforts to reform the topheavy, inefficient, and highly politicized Spanish army.

The bulk of the book, however, deals with the reforms themselves. The longest chapter—seventy-seven pages, including tables—analyzes the linchpin of the reform program, the retirement decree of April 25, 1931, which successfully tackled the principal defect of the Spanish army, the excess of officers. Alpert convincingly calculates the number of retirements at about eight thousand and thoughtfully discusses their impact on military structure. In this chapter and elsewhere, he reproduces substantial portions of the decrees in order to probe Azaña's intentions. His primary concern is to establish that, although Azaña's achievements were limited by social and economic conditions in Spain in the 1930s, his program nevertheless represented a successful effort to harmonize Spain's military policy with its new republican institutions and laid the groundwork for the modernization of the army by subsequent ministers.

Unfortunately, Alpert is not satisfied to present a reasoned justification of Azaña's reforms but writes primarily to refute the charges leveled against them by military critics and conservative historians. His heated apologetics occasionally lead him to defend decisions that on further analysis appear questionable, such as Azaña's promotion policies, or his retention of the *cuota* system. It also places the book in the tired tradition of Spanish historical writing, in which the Civil War is fought all over again.

Despite Alpert's admirable attempt to provide the background for the reforms of 1931–32, one misses an analysis of the political context in which they were carried out. In formulating his policy Azaña faced a number of political constraints, some self-

imposed, others imposed by his military supporters or by his Socialist allies in the republican coalition. The omission of these factors at times renders the explanation of Azaña's successes and failures incomplete.

More importantly, Alpert does not address the major question confronting the student of Spanish civil-military relations: Why did over half the officer corps rebel in 1936? One suspects that Alpert would argue that the army's professional grievances played an insignificant role in the origins of the Civil War, but this begs the question of the bitterness of the military attacks against the minister and his program. In any event, by avoiding the question altogether, Alpert fails to relate his study to the theoretical issues that social scientists and historians have debated in recent years.

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PETER WALDMANN, *et al.* *Die geheime Dynamik autoritärer Diktaturen: Vier Studien über sozialen Wandel in der Franco-Ära*. (Schriften der Philosophischen Fakultäten der Universität Augsburg, number 22.) Munich: Ernst Vögel. 1982. Pp. xvii, 404. DM 48.

This volume consists of four discrete monographs dealing with the recent social and political change in Spain. The title begins uncertainly, for it is quite doubtful that any "secret dynamics" remain to be discovered concerning the Franco regime, which became possibly the most thoroughly studied dictatorship in the world before it came to an end. Moreover, the scope of these monographs is in fact broader than the subtitle—"Social Change in the Franco Era"—might indicate, for they extend into the first major phases of the post-Francoist period, as well. They are happily all of high quality and, because of their distinct themes and character, can best be discussed individually.

The first and briefest, by H. C. Felipe Mansilla, treats the changing political and institutional role of the Spanish military through the long Franco era and into the first phases of the democratic regime. It fails to break new ground, since the Spanish army has already been subjected to a certain amount of political and institutional study, even the most recent period having been treated in Carlos Fernández's *Los militares en la transición política* (1982). It is nonetheless lucid and surehanded, correctly identifying the principal factors that influenced changes in civil-military relations and within the armed forces themselves and thus presents a reliable analytical synopsis for the period treated, effectively demolishing the Spanish leftist concept of "military fascism."

The longest of the four works is Walther Bernecker's study of "The Worker Movement under Francoism," whose title might have been more accurately rendered in the plural. It consists of three different sections that treat the early leftist worker resistance, the development of the regime's own state syndical system, and finally the emergence and growth of the later worker opposition together with the transition into new democratic structures. Such topics are extraordinarily complex and can be treated only in general terms. The first part consists essentially of a lucid summary of information otherwise available, but Bernecker's treatment of the Spanish state system and of the later worker movements presents and analyzes data less readily accessible and thus constitutes a genuine contribution to contemporary Spanish social history.

Peter Waldmann's inquiry into "Socioeconomic Change, Centralist Oppression, and Protest-Violence in the Basque Country" discusses the growth of the Basque terrorist movement *Euzkadi ta Azkatasuna* (ETA) during the 1960s and 1970s within the context of the recent history and socioeconomic changes of the Spanish Basque country. Its ultimate goal is to explain the escalation of terrorism in one of the most concentrated industrial zones of Europe amid a context of increasing democratization, conditions almost the opposite of those sometimes held to account for widespread political violence. Waldmann's conclusions are cogent and convincing. Though too complex to be briefly recapitulated, they interpret the development of the ETA phenomenon through the dynamic interaction of a network of interrelated social, economic, cultural, political, and historical factors and provide the best brief analytic synthesis of this problem that has been presented to date.

The fourth monograph, Francisco López-Casero's "The Generation of Revolution (*Umbruchs*)," studies the recent transformation of economic relations, lifestyle, and social psychology in the author's native Campo de Criptana, an "agrarian city" of some fourteen thousand in La Mancha. Based on the author's earlier unpublished dissertation of 1968, it presents an interesting and indeed somewhat novel analysis of the effect of drastic socioeconomic transformation in rural Spain. Most such studies emphasize the highly disruptive effects of recent alterations and in many cases the virtual dissolution of local rural society. López-Casero's account reveals drastic economic structural changes and profound alterations in social psychology, but it also presents a portrait of a sizable rural community that has profitably adjusted to the new economic structure without losing its collective identity or coherence and thus stands in interesting contrast to many other recent sociological and anthropological studies of rural Spain.

These varied monographs cannot form a unified whole, but their solid research, objective discussion, and analytical sophistication present eloquent testimony to the quality of research on contemporary Spain currently being conducted in West Germany.

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JONATHAN I. ISRAEL. *The Dutch Republic and the Hispanic World, 1606–1661*. New York: Clarendon Press of Oxford University Press. 1982. Pp. xvi, 478. \$54.00.

The historiographical origins of this book lie in the program set forth by J. P. Cooper in volume four of *The New Cambridge Modern History* for marrying the unlikely pair of *Annales* and narrative history into a single historical mode. Jonathan I. Israel, who teaches at University College, London, admits that this attempt, which he defines as "the analysis of political events in the light of economic trends and vice versa," is highly unusual, and a reviewer can add that it is highly praiseworthy. It is a difficult task under any circumstances, and one made all the more so here by having as its subject the relations of the Dutch republic and Spain during the first six decades of the seventeenth century. Beyond the obvious doubling of the field, such a study requires particularly deep and subtle knowledge of institutions and events in each of the countries involved. Furthermore, few historians, even specialists in early modern Europe, know the history of the two countries in the depth and with the fullness that they would usually command for, say, France or England, and therefore the background cannot be taken for granted in the same way. This reviewer, for instance, is only too aware that his judgment on the author's command of the Spanish side is much thinner than on the Dutch. Finally, effective narrative requires a very different form of exposition than structural description and analysis.

It is therefore not a contradiction to say that the author has not fully succeeded and that his book is a significant and valuable achievement. Its greatest strength lies in its rich exploitation of the archival sources, deepening our knowledge well beyond the printed sources, although these are not at all neglected. The secondary literature is drawn on for data rather than for argument. Historiographical debate is implicit rather than stated forthrightly; those familiar with the literature will recognize revisions and reversals of the established picture, while others will merely see what the author has painted. The picture, despite the wealth of detail, is essentially limited to politics and economics, both narrowly conceived. The narrative mode is not one

the author handles with ease; his account lacks smooth flow, and his prose is awkward, sometimes to the point of embarrassment. (The atrocious proofreading must presumably be blamed on the press; I cannot believe that the author would have permitted line-end hyphenation in the middle of a consonant cluster, as happens repeatedly.)

The essential contribution of the book consists of two points. The first is the role of the Dutch war of independence in creating, as it were, a normality out of abnormality for the economic life of both countries. The war lasted so long that different regions and branches of the economy adapted to new conditions as if they were permanent and used all their political strength to defend their positions. It was easier on both sides to defend continuation of the war against an ideological and national enemy than to argue in the face of such considerations of higher morality for a return to a normality that had not existed for decades. The second contribution is to see events from a new angle, at once economic and political. This is a book that prompts rethinking of important issues. It would have been more effective still if it did not systematically minimize psychological and ideological dimensions. For instance, one may properly "relativize," as the Dutch would say, or even reject outright Geyl's "Great Netherlands idea" in studying the relation of the northern and southern, Dutch and Spanish, Netherlands; but it hardly is justified to neglect it without a single mention. The author rejects the Marxist relationships between economics and politics as infra- and super-structure, but he himself conceives them too thinly. The interplay of economic theory and empirical research that marks the work of such historians of the Dutch as Jan de Vries and of the Spanish as David Ringrose is missing.

One last point: the story is told alternately from the Dutch and Spanish sides, rather than being woven into a single account. If it had been told essentially from one side, it would have been easier to follow, even though the range of Israel's erudition and research might have been less obviously stunning. We still should be grateful for the book he chose to write.

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GERROLD VAN DER STROOM. *Duitse strafrechtspleging in Nederland en het lot der veroordeelden* [German Administration of Criminal Justice in the Netherlands and the Fate of the Convicted]. (Rijksinstituut voor Oorlogsdocumentatie, Cahiers over Nederland en de Tweede Wereldoorlog, number 4.) The Hague: Staatsuitgeverij. 1982. Pp. 118.

During their occupation of the Netherlands during the Second World War, the Germans set up their own judicial system to punish transgressions by Dutch citizens directed against the occupying power or against wartime regulations. In addition to the military court system that became progressively less important, *Reichskommissar* Seyss-Inquart established a civil judiciary system. The ordinary German court system had two tiers, the *Landesgericht* as a lower and the *Obergericht* as the higher court. The latter could under certain circumstances sit as a summary court or *Sondergericht* following an abbreviated set of procedures. In addition to these civil courts, the Germans also set up special SS and police courts to deal primarily with resistance and other political activities endangering law and order. There was, however, a certain degree of arbitrariness governing the assignment of the cases of suspects to these various jurisdictions. The *Reichskommissar* retained ultimate control over both civil court systems, which continued to operate until the end of the occupation.

The first part of Gerrold van der Stroom's study deals primarily with the operation of the *Obergericht* (*Sondergericht*), although he presents also a brief summary of the decrees affecting the operation of the other court systems. The second part describes the life in German prisons and penitentiaries of Dutch patriots condemned by German courts to terms of varying length. A brief summary in English concludes the booklet.

An estimated sixty thousand Dutch nationals were apprehended by the German police during the occupation. Of these the *Obergericht* or *Sondergericht* tried 2,213 people pronouncing 112 death sentences. Most of the convicted Dutchmen (and women) were sent to prisons and penitentiaries in the Rhineland. These were administered by the German Ministry of Justice, which meant that the prisoners were out of the hands of the SS and police for the duration of their incarceration.

Conditions in German prisons were harsh but tolerable for most of the war, but they grew much worse after August 1944, when penal institutions in the path of Allied armies began to be evacuated. Instructions were issued to hand over political prisoners to the SS and police if they could not be relocated conveniently or, if neither was possible, to kill them. In the final months of the war, the SS demanded in a number of instances that political prisoners be handed over to them. Prison directors varied in their reactions. Some collaborated, while others practiced tactics of delay, thereby allowing a majority of Dutch inmates in German prisons to survive the war.

The first part of the study, with its catalogue of German laws and decrees and of judicial systems and their operation, may be useful for reference purposes; but it does not offer much basically new

material. The second part, dealing with prison life in Germany, tells a fresh and comprehensible story. It shows that those who were sentenced to definite prison terms in German penal institutions found themselves in a more tolerable situation than those of their compatriots who were consigned to a concentration camp by the German police.

WERNER WARMBRUNN
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PETER UFFE MEIER. *Omkring de fire species: Dansk merkantilistisk stabel- og navigationspolitik i 1720' rne* [The Four Commodities: Danish Mercantilist Staple and Navigation Politics in the 1720s]. Summary in English. (Institute for Økonomisk Historie, Københavns Universitet, number 17.) Copenhagen: Akademisk Forlag. 1981. Pp. 176. 65 KR.

This work, by Peter Uffe Meier, is an interesting study of the step-by-step development of the Act of the Four Basic Commodities (*Forordning om de fire species*) enacted by Denmark's government in 1726. The law gave Copenhagen merchants a monopoly on the maintenance of bonded warehouses and control over the importation of wine, distilled spirits, salt, and tobacco.

Denmark-Norway's government first emphasized the fiscal importance of the four commodities and showed commercial favoritism to Copenhagen through legislation enacted in 1688 and 1691. Domestic trade, however, did not prosper because of the Great Northern War, smuggling, and Dutch competition.

When Denmark's commercial treaty with the United Netherlands expired in 1721, Copenhagen merchants therefore repeatedly proposed to the Board of Trade (*Politik-og kommercekollegiet*) that Copenhagen be established as the sole staple for the four basic commodities. They contended that foreign middlemen would be eliminated by securing these goods from the country of origin. Provincial merchants would not be harmed by this measure, it was claimed, because they did not trade extensively abroad; smuggling would be reduced; and a significant increase in trade, and therefore in the nation's wealth, would occur.

The government was not easily convinced even though it had already shown a preference for mercantilistic legislation. Although two governmental bodies concurred separately to this proposal in 1723 and 1724, the Royal Council (*Konseillet*) and King Frederik IV rejected it on both occasions. They reversed themselves, however, on June 1, 1726, when the above law was proclaimed, but conditions were placed even then on the staple to protect the consumer, Danish shipping interests, and provincial merchants. Meier, relying chiefly on

archival material from the Board of Trade and Ministry of Finance (*Rentekammeret*), is not able to explain the reasons for this change but suggests that Sweden's enactment of its own Commodity Act (*Produktplakaten*) influenced Danish policy.

This new policy did not live up to the expectations of either the government or Copenhagen's merchants. The staple could not meet the conditions imposed on it by the government, and Copenhagen's trade did not increase as rapidly as expected. Moreover, Christian VI was less sympathetic to the monopoly than his father had been, rescinding it therefore soon after he became king in 1730.

Meier's work is an examination of a policy that failed quickly. He concludes that Copenhagen's merchants lacked the resources to fulfill the program they had sought for such a long time. The book, nevertheless, is an important study of mercantilistic principles and practices. Although not intended as such, it also provides an interesting look at the decision-making process followed by the Danish government. Officials obviously favored Copenhagen mercantile interests by the legislation of 1726 but only after considering the views of Denmark's provincial towns, Norway, and other groups. This in turn encumbered the law with so many limitations on Copenhagen's privileges that this mercantilistic experiment failed. The five-page English summary provides a valuable overview of the book's contents and conclusions for those who do not read Danish.

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MIRIAM USHER CHRISMAN. *Lay Culture, Learned Culture: Books and Social Change in Strasbourg, 1480-1599*. New Haven: Yale University Press, 1982. Pp. xxx, 401. \$35.00.

MIRIAM USHER CHRISMAN. *Bibliography of Strasbourg Imprints, 1480-1599*. New Haven: Yale University Press. 1982. Pp. xxi, 418. \$35.00.

From the first page of this exciting book one is aware that it is a major work of scholarship. Using a computerized listing of total book production in Strasbourg between 1480 and 1599, Miriam Usher Chrisman analyzes in detail both the city's book trade and reading public. In part 1 she takes us skillfully into the intricacies of the production process, providing much valuable information about the working of printing firms of all sizes. Her discussion is supported by over forty informative graphs and tables, while the companion volume supplies a comprehensive bibliography of the books concerned. Arranged according to the major analytical themes of Chrisman's wider discussion of the cultural impact of printing in Strasbourg, this is an invaluable

tool for scholars. It is marred only by failing to show the format and size of the items listed, so that we are left to guess whether an individual entry is a broadsheet or a weighty folio tome.

Assembling this data is a major achievement in itself, but the bulk of the book is concerned with a stimulating and original analysis of the evolution of learned and lay culture in sixteenth-century Strasbourg. Chrisman's thesis is that there were two cultures, lay and learned, corresponding to the two major languages of publication, German and Latin. Learned culture, essentially a clerical culture, predominates at the beginning, but it is diversified during the sixteenth century into a composite culture of scholars, scientists, and theologians. The vernacular lay culture is that of the vigorous artisan and burgher classes, which challenges, and to some extent overtakes, clerical culture before and during the Reformation. It is unable, however, to break down the barriers between itself and the Latin-reading culture of the learned. It triumphs briefly, experiencing a "brief cultural revolution" in the years from 1549 to 1570, but learned culture remains the dominant culture. Its hegemony is built on the Latin-based, humanist-influenced education of the Strasbourg Academy, a form of education that creates "a common cultural experience for the governing classes of Western Europe" and that forges "new bonds of loyalty between the secular and spiritual leaders of the community" (p. 259).

Lay vernacular culture was a distinctive creation of the urban milieu. In religion, it manifested itself in demands for a simple and practical faith, without too much doctrinal complexity. It had an Erasmian, ethical view of Christ as a man among men, acting in the world, rather than the more subtle and abstract view of him as Redeemer found in Protestant theology. In secular matters, it exemplified a strong work ethic and a drive toward self-instruction, shown in the popularity of numerous how-to-do-it books. It rested on a popularized, new, scientific world view, with an awareness of a dynamic natural world capable of being mastered, or at least understood, by the application of practical knowledge. It even had what can only be called a "burgher ideology," expressed in popular novels and theater, which upheld the merits of burgher life in direct opposition to aristocratic values and sought to establish a bourgeois family ethic (the latter something virtually ignored by the new married Protestant clergy once they had settled their own problem of celibacy).

This book will provoke a good deal of discussion on several themes in the social, cultural, and religious history of the sixteenth century. It upholds the socially conditioned nature of the spread of ideas, although it is ambiguous about the causal relationship between the Reformation and the triumph of lay culture. It rejects the myth of "civic unity" in the

sixteenth-century town, arguing that social and cultural diversity was the norm before, during, and after the Reformation. It supports the view that the Reformation was only a limited success and had rather different outcomes for the cleric and lay person. It also introduces the fruitful suggestion that Strasbourg's cultural evolution in the period resembles the social anthropologists' notion of a "revitalization movement." Although Chrisman does not suggest it, her reading of the changing relationship of learned to lay culture comes close to Gramscian ideas about the pattern of dominant and subordinate cultures. Finally, it raises the issue of the emergence of "middle-class" consciousness and ideology during the sixteenth century.

There are a few minor reservations about the picture Chrisman draws. She overlooks too easily the problem of how extensive literacy was in Strasbourg. On the one hand, her estimate that "books were in the possession of over 40 percent of the citizens, including artisans and workmen" (p. 69) rests on the rather unreliable evidence of will inventories. On the other hand, she ignores the evidence of a flourishing oral culture. Seventy-five percent of early vernacular publication was drawn from this oral culture, and its continued vitality is reflected in the popularity of vernacular theater and songs, as well as in the fact that most people knew their Bible more from hearing sermons and singing the Psalms than from reading. Here the pertinent question is, Who were the mediators who acted as brokers between oral and literate culture? Is it possible that like persons mediated between lay and learned culture? Here Chrisman may have set up overly rigid categories at the outset, which may have determined the outcome of her analysis. Such queries will do nothing to detract from a very fine book, which maintains its sense of excitement and discovery to its very end.

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JAMES MARTIN ESTES. *Christian Magistrate and State Church: The Reforming Career of Johannes Brenz*. Buffalo: University of Toronto Press. 1982. Pp. xii, 190. \$27.50

Johannes Brenz (1499–1570), the reformer of Schwäbisch Hall and a moving force in the institutionalization of the Lutheran movement throughout south Germany, especially in Württemberg, was one of those figures whose thought and impact has lain largely hidden by Luther's shadow from modern students of the Reformation. James Martin Estes provides a thoroughly researched and cogently argued interpretation of a major aspect of Brenz's thought.

Brenz influenced the theology of Lutheran orthodoxy to a great extent through his friend and disciple, Jacob Andreae, especially in its Christology. But that is not Estes's subject. Rather, Estes analyzes Brenz's understanding of the relationship between church and state. Again, it was Andreae, through his efforts in forging the Concordian settlement of 1580, who combined Brenz's prescriptions for this relationship with those of some north Germans and who thus endowed German churches with this Brenzian model. Estes notes but does not trace Brenz's influence. Even more elusive are the antecedents of his viewpoint: beyond Luther's influence Estes is not able to delineate the intellectual sources of Brenz's thought and thus to explain why Brenz diverged significantly from Luther and some other Lutheran reformers on a number of issues related to church and state.

The simplicity of Estes's concluding judgment that "Brenz was a state-church man because historical circumstances hardly allowed him to be anything else" (p. 144) belies Estes's own finely tuned analysis of Brenz's thought. Within a range of Lutheran views of church and state Brenz did make "conservative" choices that favored centralization and a strong role for secular rulers within the church. Only on the subject of toleration of Anabaptists was Brenz relatively "liberal."

Brenz did strongly favor an active role for the ruler within the church, but he was not sycophantic. He tried to safeguard the pastor's right to exercise his ministry of the Word freely and to preserve the financial integrity and independence of the church. And, as warm a supporter of the prince as he was, he opposed active resistance of inferior magistrates to the emperor, even though he worked out a somewhat dubious defense of Schwäbisch Hall's entry into the Schmalkaldic League.

Also of interest is Estes's analysis of Brenz's attempt to establish a workable, centralized instrument for enforcement of high moral standards in Württemberg. The best efforts of pious theologians and sincere princes could not eliminate vice. Estes's description of Brenz's goals for the moral order raises the question of just how different Brenz's ideal society was from that of Calvin. Were the differences between Geneva and Württemberg more due to political structure and size than to the form of the moral order envisioned by the reformers or to their fervor?

Estes's competent and thorough study of Brenz's ideas and actions offers a solid contribution to the advancement of our understanding of the Reformation and its impact on four centuries of German church life.

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REINHARD HEYDENREUTER. *Der landesherrliche Hofrat unter Herzog und Kurfürst Maximilian I. von Bayern, 1598–1651*. (Schriftenreihe zur Bayerischen Landesgeschichte, number 72.) Munich: C. H. Beck. 1981. Pp. xl, 381.

Forty years ago, H. F. Schwarz, in *The Imperial Privy Council in the Seventeenth Century*, pioneered a prosopographical approach to the administrative history of an Austrian Habsburg organ of government. That that approach is still very much alive is demonstrated in the work under review. There have been a number of studies in the last two decades on the early modern Bavarian state from the pens of historians like Heinz Dollinger and Dieter Albrecht, Andreas Kraus and Hans Rall, the last of whom supervised this exemplary, archive-based study, accepting it for the Munich PhD in 1978. Reinhard Heydenreuter is to be congratulated for plugging the gap in our knowledge regarding the fiscal, judicial, and administrative arrangements that made the Counter Reformation state of Maximilian I such a prime force in the build-up toward those conflicts known as the Thirty Years' War. We now know even more why Maximilian could sustain his aggressive, Leaguist stance and survive the Swedish incursions of the 1630s.

Here, then, is a rather dry but important work on the development of Bavarian central government as a court council mediating between the estates and the main executive organs of chancellery, judiciary, fisc, and civil list. It handles the sixteenth-century background, especially the reforms of Albrecht V and William V, and lets the reader contrast their more modest but essentially sound achievements with those that followed in the long and powerful reign of Maximilian I, who emerges as a paternalistic, bigoted, at times tyrannical, but also hard-working and utterly consistent ruler, who left nothing to chance or to providence in training, hiring, and supporting the several hundred minor dynasties of officials that ran his model, absolutistic, Catholic state. Heydenreuter has reconstructed it all meticulously from the archives, and his scholarly thesis marshals evidence for the specialist in early modern German history. For the general historian, the best starting point is an essay by Heydenreuter, "Die Behördenreform Maximilians I," which was published in a magnificent exhibition catalogue, *Wittelsbach und Bayern*, volume 2:1, *Um Glauben und Reich: Kurfürst Maximilian I* (1980).

GERHARD BENECKE
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GERHARD MENK. *Die Hohe Schule Herborn in ihrer Frühzeit, 1584–1660: Ein Beitrag zum Hochschulwesen des deutschen Calvinismus im Zeitalter der Gegenreformation*.

tion. (Veröffentlichungen der Historischen Kommission für Nassau, number 30.) Wiesbaden: The Commission. 1981. Pp. ix, 363.

German Calvinism in its formative phase has been poorly served by historians. It has usually been identified with the political militancy of the Rhenish Palatinate and judged by the costly failure of the palsgrave at the beginning of the Thirty Years' War. Gerhard Menk's book offers a different perspective. He plays down the degree of political solidarity achieved among the Empire's various Calvinist territories, arguing instead that it was the cultural sphere that provided their most homogeneous and significant contribution to the history of the period. He identifies as their intellectual powerhouse the little university town of Herborn, in Nassau.

Herborn was not, strictly speaking, a university at all, but a *Gymnasium illustre*, or high school, without imperial charter, like the Strasbourg academy that it took for its model (and that likewise has earned, in A. Schindling's *Humanistische Hochschule und freie Reichsstadt*, an excellent recent monograph). Yet that did not hinder Herborn's role, either as training ground for the Reformed clergy of northwest Germany or as the goal of students from many parts of the Empire and beyond. Menk patiently charts the complicated institutional history from the foundation of the school by Count Johann VI of Nassau in 1584 (the very year his more celebrated brother, William the Silent, was assassinated) through its almost total collapse during the Thirty Years' War to the feeble attempts at recovery from the 1630s. His account really comes alive, however, in the lengthy sections devoted to Herborn's notable professoriate: Olevian, the formulator of covenant theology; Piscator, the controversialist and commentator; Zepper, the theorist of ecclesiastical society; Althusius, who accommodated the rival claims of estates and princely sovereignty; and above all Alsted, the famous encyclopedist. These men, as the author shows, all asserted a distinctively Calvinist brand of learning, which, in a direct and practical manner, proceeded from particular to universal like the Ramist method that all of them embraced. Their message correspondingly found its way to every part of the Calvinist globe, from Transylvania to New England.

Menk's study has its weaknesses. His own rather wearisome organization involves some repetition, and he concludes firmly *in mediis rebus*. His vague, portmanteau understanding of Ramism begs too many questions. His account of Herborn's impact on Bohemia and Hungary, while welcome, leads into error (like a confusion between Pavel Jeřín and Jan Jessenius) and oversimplification: Did not Comenius's pedagogical ideas also owe much to the native tradition? But on the whole this is a book that deserves a wide readership, above all for its central

thesis that the true "Calvinist international" was a body of scholars and teachers, reformulating the universal, late humanist intellectual world in the image of the Calvinist territorial state.

R. J. W. EVANS
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GÜNTHER GRÜNTAL. *Parlamentarismus in Preussen, 1848/49–1857/58: Preussischer Konstitutionalismus—Parlament und Regierung in der Reaktionsära*. (Handbuch der Geschichte des Deutschen Parlamentarismus.) Düsseldorf: Droste. 1982. Pp. 539.

This is yet another fine volume in the excellent "Handbuch der Geschichte des Deutschen Parlamentarismus" series. It fits chronologically between Manfred Botzenhart's *Deutscher Parlamentarismus, 1848–1850* (1977) and a work on the Prussian *Konfliktzeit*, which has yet to appear. For that reason alone it is an especially useful volume. As the author says, "the history of Prussia in the 1850s still counts . . . as terra incognita" (p. 12). This particularly bleak and unfruitful period is usually treated mainly as a prelude to Bismarck. Günther Grünthal tries to present the issues of the 1850s in their own context. Yet the larger questions do intrude. Most of the book addresses the issue of whether Prussia was a constitutional state at this time—not only in the sense of a state in which the political authorities were bound by a fundamental law, but also in the sense of exemplifying the particular type of regime that the Germans called "*konstitutionell*"; neither absolutist nor democratic nor yet a transitional system tending toward the one or the other, but rather an inherently stable regime in which political power was neatly divided between a monarchical executive and an elected legislature. This question has most recently been debated by Ernst Rudolf Huber, Ernst-Wolfgang Böckenförde, and Hans Boldt, among others. Grünthal's contribution to the discussion suggests that the Prussia of the 1850s is not a good place to look for real constitutionalism in either of these senses. He depicts it as a regime in permanent crisis that never really acquired a distinctive identity. To the paradox of a constitution that gave virtually absolute power to the king in the form of unlimited emergency powers was added the paradox of a Junker opposition that effectively defeated constitutionalism by constitutional means, thus paralyzing the government and blocking the way to further development. The constitutional crisis solved by Bismarck seems to date not from the liberal electoral victory of 1858 but from the moment the constitution was promulgated in December 1848.

I think a *Handbuch* is supposed to be a sort of reference work in which individual sections make

sense even if they are consulted separately. Only the last third of this book answers that description, dealing as it does with such parliamentary arcana as standing orders and party organization as well as with the elections of 1852 and 1855. It is no negative reflection on the author to say that he has not entirely succeeded in his stated objective of "analyzing the relationship between constitutional system and social system" (p. 11), since neither the political nor the social history of the 1850s is yet sufficiently well researched to make that possible in a very satisfactory way. Even the elections of that decade have received very little scholarly attention. Grünthal promises to publish in a future volume some valuable social statistics that the Prussian Statistical Bureau assembled in an attempt to anticipate the effects of different electoral strategies. His welcome contributions go a long way toward illuminating this historical *terra incognita*.

DONALD J. MATTHEISEN
University of Lowell

RUDOLF FORBERGER. *Die Industrielle Revolution in Sachsen, 1800–1861*. Volume 1. Part 1, *Die Revolution der Produktivkräfte in Sachsen, 1800–1830*; part 2, *Die Revolution der Produktivkräfte in Sachsen, 1800–1830: Übersichten zur Fabrikentwicklung*, compiled by URSTULA FORBERGER. Berlin: Akademie. 1982. Pp. xi, 613; viii, 234. 150 M the set.

Frequently there is a tendency to generalize from the experience of Prussia for all of Germany. Rudolf Forberger, a well-known interpreter of the economic history of Saxony, tries in part to rectify this situation by pointing to Saxony's pioneer role in the industrial revolution of Germany. Nevertheless, this work is not an example of comparative history. The emphasis throughout is on Saxony, with little effort made to contrast its development with that of its neighbors such as Prussia or Bohemia.

These are the first volumes of what has been announced as a multivolume work. Actually, the second volume of the present work is no more than an extensive list of all the factories in Saxony between 1800 and 1830 for which a record exists. This period of time is considered by Forberger to be the first phase of Saxony's industrial revolution. The volume that is to follow this work will presumably take up the remaining phase, which lasts until about 1860.

The text volume of the present work is a thorough, traditional treatment of the subject. It relies strongly on archival sources and material that was printed in the nineteenth century. The frequent excerpts from these sources lend greater authority to the work but tend to make for slower reading.

There is apparently an absence of private records

of enterprises in Saxony for the period 1800 to 1830. The reports of the *Landes-Oeconomie-, Manufactur- und Commerzien Deputation*, the government agency whose apparent task was to encourage industrial development, serve as a major source for the book. Saxony paid premiums to inventors and innovators and at times provided loans to enterprises. For the rest, government policy was liberal in the nineteenth-century sense of the term and adhered to free-trade principles. It left industrial development much more up to private initiative than was the case in the neighboring areas of Prussia and Bohemia. Nevertheless, as in those places, Forberger shows, a considerable effort was made in Saxony to introduce technology from abroad. Interestingly, the United States served as an important intermediary for this purpose. It was easier to get new machines from there than from England. Forberger devotes a considerable portion of his work to the question of technological transfer. Not surprisingly he finds that there was a sizable time-lag in the adoption of English innovations in Saxony. To be sure, in the case of the steam engine, as an example, the availability of water power in the *Erzgebirge*, one of Saxony's major industrial regions, delayed its strong and early introduction.

Forberger's is a work of great detail. The author seems to have canvassed the primary and secondary sources with much industry. It goes without saying that studies of industrial development in nineteenth-century Germany will be difficult indeed to compose without consulting these volumes.

H. FREUDENBERGER
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VOLKER DORSCH. *Die Handelskammern der Rheinprovinz in der zweiten Hälfte des 19. Jahrhunderts: Eine Studie zum Funktion und Entwicklung wirtschaftlicher Interessenvertretungen*. (Frankfurter Historische Abhandlungen, number 24.) Wiesbaden: Franz Steiner. 1982. Pp. ix, 199. DM 44.

In the recently mushrooming political histories of organized economic interest groups during the *Kaiserreich*, the Prussian chambers of commerce, *Handelskammern* (HKs), have played a minor role and are judged to have been ineffective. Now, almost paradoxically, Volker Dorsch manages to squeeze an interesting story from a seemingly unremarkable event: the survival of these bodies during the decade of the 1880s. The interest in the story derives from the fact that Bismarck's politics after 1878, when he turned toward economic protectionism, impinged on the HKs in two diametrically opposing ways. His interventionist policies needed the HKs, yet he sought to do away with them. Thus their fortunes during this decade provide clues about how the first

trial-and-error period of state intervention into the imperial economy functioned.

The policies of state intervention—for example, tariff protection, railway nationalization, the sanctioning of cartels—required monitoring by a stream of detailed, reliable economic reports. In this respect the HKs proved to be peerless, since neither local government, the protectionist *Verbände* (national business associations), nor the *Verbände*-influenced *Volkswirtschaftsrat* (VWR) could generate equivalent data. Dorsch argues that local governments were hampered by the inadequate, exclusively legal training of their bureaucrats, while the counsel of the *Verbände* failed the test of quality. The Ministry of Commerce increasingly doubted the economic objectivity of these sources. Thus, the HKs' information monopoly was a decisive asset guaranteeing their continued existence.

At the beginning of the decade, however, when Bismarck was still setting up his new protectionist governing coalition of rye and iron and the HKs got in the way, their prospects did not look rosy. The thorn in Bismarck's side was the fact that many HKs remained bastions of free trade and continued to espouse such heresy in their published annual reports to the Prussian Ministry of Commerce. Bismarck's HK-disciplining edict of November 1881, which required submission of the reports to the Ministry for correction, that is, censorship, before publication, effectively muzzled this important source of liberal ideas. Consistent with this policy was the building of a new organizational bridge to protectionist big-business interests and their *Verbände* by the creation of the VWR (1880–92), a corporative consulting body that was to assess pending legislation affecting business and agrarian interests. Finally, Bismarck espoused the establishment of a regional base to the VWR in the *Gewerbekammern*, a step that directly threatened the existence of the HKs. Nevertheless, the initially attractive political quality of these new corporative bodies for Bismarck, their manipulability, conspired with other weaknesses in these organizations, especially their lack of economic competence, to discredit them in the public's eye. By 1890 their end and that of the era of organizational competition and confusion was in sight.

Dorsch tells this story, which highlights the organizational failures of the early *Interventionsstaat* under Bismarck, from the point of view of the *Rheinprovinz*. This is an admirable device, since the industrialized Rhineland was a crucible for most of the relevant economic and political developments of the *Kaiserreich*. The province was also the historical base of the HK movement in Prussia and had a dense network of chambers. An advantage of this regional approach is that it permits a clearer understanding of the complex communications network

and the interdependencies between electoral politics, bureaucracy, *Verbände*, the HKs, and their corporative challengers. In the parallelogram of forces involved in this system of interest articulation, the corporative bodies proved to be redundant in contrast to both the *Verbände* and the HKs, which played essential roles.

Although the economic history of this study is a bit flawed—for example, the author gets into a number of needless complications due to his assumption that Germany entered a long phase of “great depression” after 1873 and needed protection—and the actual investigation of economic policy formation is largely abandoned in favor of spreading out institutional details from the archives, Dorsch's study breaks new ground as a solid organizational history.

ROLF HORST DUMKE
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WERNER K. BLESSING. *Staat und Kirche in der Gesellschaft: Institutionelle Autorität und mentaler Wandel in Bayern während des 19. Jahrhunderts*. (Kritische Studien zur Geschichtswissenschaft, number 51.) Göttingen: Vandenhoeck und Ruprecht. 1982. Pp. 422. DM 98.

In this densely argued essay, one of the series of Critical Studies in Historical Science edited by, among others, Hans-Ulrich Wehler, Werner K. Blessing presents an often stimulating analysis of relationships between institutional authority and popular mentality in nineteenth-century Bavaria. Blessing's primary purpose is to determine how, and to what extent, the state and the churches shaped the mental universe of the Bavarian *kleine Leute*, here loosely defined to encompass peasants, petty tradesmen, and dependent lower middle classes. The result is a study that is part institutional history, part intellectual history, and part history of popular culture. In its regional focus and methodological self-consciousness the book reflects many of the central concerns of the German brand of social history to which the Wehler series has become a monument of sorts in recent years.

During the first half of the nineteenth century, Blessing suggests, a complex and often contradictory set of factors combined to mobilize both throne and altar in a socialization project aimed at exploiting a traditionalist, patriarchal image of Bavaria based on personal piety, social quiescence, and dynastic loyalty. After mid-century, however, this configuration of influences began to shift and eventually to dissolve. The unification of Germany around Protestant Prussia heightened confessional tensions while fostering a new bourgeois cult of the nation that subsumed, if it did not fully displace, the

specifically Bavarian cult of the Wittelsbachs. Meanwhile, urbanization and economic development were eroding those structures of community historically conducive to social and religious traditionalism. As symbolic and ceremonial forms of value representation declined in efficacy, the schools, no longer mere adjuncts of clerical or dynastic authority, increasingly assumed prominence as agencies of socialization. By 1914 the general emancipation of civil society had advanced beyond the control of the old institutions of order. New associational and propaganda initiatives enabled churchmen, especially Catholics, to retard the pace of mental change to some extent, but only at the price of growing social encapsulation.

Although intentionally ignoring such influences on popular outlook as the family, work, and mass communications, Blessing's analysis gains in precision what it loses in comprehensiveness, and in so doing it makes a useful contribution to ongoing interdisciplinary debates over the nature and limits of secularization. The special value of the book lies in the skill with which Blessing conceptualizes and charts the historical logic of a process that, as he repeatedly shows, was hardly automatic and anything but unilinear. Blessing is a sure guide through the intricacies of Bavarian culture, and he has mined a wealth of material from both archival and published sources, including a variety of local church records previously all but neglected by academic historians. Unfortunately, much of this material must be ferreted out of his copious footnotes. Blessing only occasionally ventures off the high road of generalization and synthesis to explore humbler evidentiary byways. As a result, his book rarely achieves the evocative richness and specificity that distinguish, for example, Eugen Weber's *Peasants into Frenchmen*, a study to which it bears a distant family resemblance (and one of the few titles missing from Blessing's exhaustive bibliography). This may be simply a matter of taste. Still, Blessing has an interesting story to tell, and if his book makes a predictably good case for the historian's use of social-science theories, it makes an equally good one, if often by default, for the much-bruited revival of narrative.

DAVID J. DIEPHOUSE
Calvin College

KLAUS J. BADE, editor. *Imperialismus und Kolonialmission: Kaiserliches Deutschland und koloniales Imperium*. (Beiträge zur Kolonial- und Überseegeschichte, number 22.) Wiesbaden: Franz Steiner. 1982. Pp. xiii, 333. DM 54.

One of the major gaps in the growing literature on the German colonial experience has been the lack of

a comprehensive, up-to-date treatment of German missionary activity. The gap has now been partly filled by this collection edited by Klaus J. Bade. The contributions to the volume cover a broad range of topics, from the early histories of the Protestant and Catholic missionary societies to the roles of missionaries in the individual German colonies. Some of the chapters provide important information and insights that are new or have been scattered among studies primarily devoted to other subjects.

There are, however, a number of weaknesses in the collection that prevent it from being a truly significant contribution to the study of missions and of German imperialism. Like most collective works, it is uneven in quality and less carefully focused than one might wish. More serious are the lack of evidence of intellectual exchange among the contributors, the absence of agreement about what is important in mission history, and the lack of a comparative approach. Although Bade gives a very nice summary of German colonial history and a useful general statement of issues in the history of missions, he makes no real attempt to compare the experiences of missions in the various colonies or their impacts on colonial peoples. The chapters are separate studies, largely unrelated to one another. They do not proceed to a consensus about the overall significance of the colonial missions as an aspect of German imperialism.

Most of the chapters concentrate on the organizational histories of the missions in individual colonies and on the missions' relationships with each other and with the colonial authorities. Arthur Knoll's chapter on Togo and Renate Nestvogel's on Cameroon fall into this category, although within their limits they are careful and interesting pieces. Only Peter Hempenstall, writing about New Guinea, attempts to deal at length with the complex interactions between missions and non-European peoples and the ways in which these interactions affected the relative success of the missions, the structure of colonial authority, and the processes of sociocultural change. Certain of the chapters, especially that by Lothar Engel on Southwest Africa, appear to have been written in something of a conceptual vacuum without substantial reference to other research on German imperialism.

The political element is also unevenly treated. While Bade and Horst Gründer handle the political background to the involvement of the mission societies in German colonialism in the 1880s very well, relationships between later mission activities and domestic German politics are generally mentioned only in passing. John Moses, in his chapter on Samoa, goes into the subject more deeply in his particular case, but the book would have been greatly improved by a chapter on domestic mission politics after 1900 and on the role of the missions in

the colonial reform movement just before the First World War.

WOODRUFF SMITH
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RAYMOND H. DOMINICK III. *Wilhelm Liebknecht and the Founding of the German Social Democratic Party*. Chapel Hill: University of North Carolina Press. 1982. Pp. xiv, 551. \$25.00.

This work by Raymond H. Dominick III, ten years in the making and here printed as a typewritten manuscript, makes no substantive contribution to our knowledge of Wilhelm Liebknecht and the founding of the German Social Democratic party. It is a leisurely narrative that recounts once more the party's familiar history. The framework of the party history provides a surfeit of narrative detail that makes Liebknecht's portrait appear pale and blurred. To be sure, Liebknecht emerges as the person he was: a passionately committed 1848 democrat, who early absorbed socialist ideas and fought a lifetime battle against Prussia and Bismarck's Reich, a great agitator for a unified labor movement and a skillful conciliator, who also lived long enough to be pushed rudely aside by the party bureaucrats and party theoreticians, whether right, left, or center. But the portrait lacks psychological and analytical depth. We learn little of what went on in Liebknecht's mind and heart, of his internal struggles and agonies, and of the reasons for his political choices. This is partly a matter of the sparse sources about Liebknecht the man, in spite of unpublished correspondence in the International Institute of Social History (IISH) used by the author; in part it is a matter of a missing point of view. Although Dominick writes that "from a vantage point in the latter part of the twentieth century, few matters loom larger than the evolution of Marxist theory and practice" (p. xi), he does not extensively and seriously discuss Marxist theory or the historiographic issues that have preoccupied historians of German history in recent decades. Neither does he try to apply social psychological or sociological theory, with the exception of labeling Liebknecht an "affective leader," an undeveloped notion. Instead, Dominick picks minor quarrels with half a dozen interpreters. It appears to him, for instance, that Liebknecht did work more actively for the First International than he has been given credit for and that he maintained closer ties than previously alleged with Marx after his return from twelve years of exile in London in 1862. Dominick sympathizes with his hero so much that he shows a visceral dislike for Ferdinand Lassalle and his successor Johann Baptist von Schweitzer, the founders of the other

wing of Social Democracy, and this spills over into a strongly worded critique of Gustav Mayer's excellent Schweitzer biography of 1909.

The main rationale for this work, then, seems to have been the consideration that after a Russian biography in 1968 and a West German one in 1976 it was time to add an English one. Liebknecht certainly deserves to be remembered as a "tolerant, humanitarian and democratic" founder of German Social Democracy and of the Second International (p. xi), but I would have wished for a more successful portrayal, both as analysis and as literature.

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DIETRICH GEYER. *Kautskys Russisches Dossier: Deutsche Sozialdemokraten als Treuhänder des russischen Parteivermögens, 1910–1915*. (Quellen und Studien zur Sozialgeschichte, number 2.) New York: Campus. 1981. Pp. xxi, 688. DM 138.

This rather curious book is volume 2 in the series "Quellen und Studien zur Sozialgeschichte," done by the International Institute for Social History, Amsterdam. The first 253 pages are Dietrich Geyer's thorough and skillful discussion of a brief episode in German-Russian socialist history: namely, the administration by three prominent Social Democratic party (SPD) members—Franz Mehring, Clara Zetkin, and Karl Kautsky—of a fund left to the Russian movement by the industrialist Nikolai Schmitt.

Pages 264 through 669 consist of reprints of some two hundred sixty-five documents (letters and party resolutions). All of these come from one of the Kautsky archives in Amsterdam, and anyone who has tried to read this collection previously will give grateful thanks to Geyer and the Institute for printing them in legible form. The collection does, indeed, cover the period indicated in the title, but more than one hundred eighty of the documents are from a single year—1911; thus, an already restricted subject is narrowed even more.

The curiosity of this work derives from the difficulties represented by Geyer's narrative being attached to a collection of documents that have been edited to exclude references to any but the subject at hand. This makes the four hundred-odd pages of letters and resolutions seem like highly elaborate footnotes, since the first two hundred fifty-three pages are far more than an introduction. As two separate works, each part could stand alone quite nicely, especially if the letters were presented without any editing. In this form the documents would have been more generally useful, beyond the limited subject treated here, and the narrative could have been more wide ranging.

One thing seems certain, however. Now that Geyer and the institute have provided us with not only a massive collection of documents but also an exhaustive discussion of their background and significance, this topic can be put to rest. Its inherent importance is rather exaggerated by the dramatic, or even melodramatic, elements of which it is comprised—the powerful SPD, which eventually failed to seize power in Germany, seemingly deciding the fate of the tiny Bolshevik faction, which eventually conquered not just Russia but a good deal of the world socialist movement as well. In fact this was a relatively minor episode in the histories of both parties, the outcome of which played little role in the ultimate development of either movement.

This work provides further corroboration of one fact that is becoming increasingly clear: Clara Zetkin was a person of remarkable good sense, poise, and competence. She deserves much closer study than she has received heretofore; someone should undertake a biography soon.

GARY P. STEENSON
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RÜDIGER VOM BRUCH. *Weltpolitik als Kulturmission: Auswärtige Kulturpolitik und Bildungsbürgertum in Deutschland am Vorabend des Ersten Weltkrieges*. (Quellen und Forschungen aus dem Gebiet der Geschichte, new series, number 4.) Paderborn: Ferdinand Schöningh. 1982. Pp. 232. DM 28.

In this important study Rüdiger vom Bruch sketches out the part played by the *Bildungsbürgertum* in an attempt to use German culture as an auxiliary arm of *Weltpolitik* on the eve of World War I. He relates the attachment of the *Bildungsbürgertum*—academic bourgeoisie—to the Prusso-German idea of the German Reich, which they as a class were determined to strengthen on the world-wide scene. The purpose of *Kulturpolitik* was the dissemination of *Deutschtum*—German cultural influence—abroad. Activities would be stressed at home that could have overseas potential for the spread of German culture. These activities included: politics, science, economics, literature, emigration, organizations and associations, schools, book stores, newspapers and publishing houses, theaters, art, research, missionary activities, medical humanitarian efforts, shipping, banking, industry, post offices, cable service, exchange of professors, maintenance of international understanding, and the development of an international peace arrangement to control conflicts.

This was no fly-by-night effort by groups of ragtag déclassé meeting in sleazy beer halls but included the cream of the professorial ranks in history and economics working in conjunction with

members of the German Foreign Office meeting in the salons, club rooms, and government offices of the Reich. It included Hermann Oncken, Friedrich Meinecke, Karl Lamprecht, Theodor Schiemann, and even Bethmann-Hollweg.

The effort was aimed not only at the citizens of the greater German Reich but also at the millions of Germans living throughout the world who preserved German culture. The purpose was to maintain a connection between the Germans at home and those abroad to further German intellectual and economic interests. The potential for success of such a venture was probably greater than one would imagine today had not World War I intervened. For example, consider how many millions of Germans lived in the United States who still held strong ties to the fatherland. The potential of the movement will never be known, however, because it was only reaching fruition on the eve of World War I when the *Gesellschaft für Erforschung des Deutschtums im Ausland* was founded on April 20, 1914.

Many important associations and institutes were involved in this effort. A partial listing includes: *Deutsche China-Institut*, *Deutsch-Südamerikanische Institut*, *Deutsch-Türkische Vereinigung*, *Deutsche Gesellschaft zum Studium Russlands*, *Deutschen Kolonialverein*, *Asiatischen Gesellschaft*, *Ostasiatischen Verein*, *Deutsche Handelsvertragsverein*, *Verein für das Deutschtum im Ausland*, and the *Gesellschaft für die Erziehung deutscher Auslandjugend im Deutschen Reich*.

The author also presents interesting material on the establishment of *Nord und Süd* by Ludwig Stein and the part it played in attempting to reach an Anglo-German understanding. Articles published in the June and July 1912 issues were written by those in the highest reaches of English-German government service, the academic community, and the business world. The December 1912 issue was devoted to Russia. The part played by Karl Lamprecht is covered in detail.

The author used an impressive bibliography including the *Nachlässe* of Hans Delbrück, Maximilian Harden, Paul Rhorbach, Friedrich Meinecke, Theodor Schiemann, Ludwig Stein, Hermann Oncken, and Karl Lamprecht. There is also a documentary appendix of some of the important manuscripts used.

The reader would have liked more analysis of the vast amount of material presented. Vom Bruch cannot be faulted for this, however, because he states at the outset of the work that it is not intended to be a complete study. It is anticipated that the book will act as an impetus for others to probe more deeply into the many topics presented. Those who do will be indebted to vom Bruch for performing the initial spadework.

ANDREW R. CARLSON
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ALBERTO MONTICONE. *Deutschland und die Neutralität Italiens, 1914–1915*. Translated from Italian. (Veröffentlichungen des Instituts für Europäische Geschichte Mainz, Abteilung Universalgeschichte, number 12.) Wiesbaden: Franz Steiner. 1982. Pp. xii, 280. DM 48.

This is an abbreviated translation of the original Italian edition published in 1971 in Bologna under the title *La Germania e la neutralità italiana*. The study, based on a thorough investigation of German and Italian archival sources, treats the albeit fruitless attempts of the imperial German government to secure Italian neutrality in World War I. Already in the summer of 1914, Berlin made an effort to railroad the Italians into joining the Central Powers. When blunt pressure failed to produce the desired results, the Germans turned to the more traditional diplomatic approach of negotiation and barter. The emissary selected was the wily old Bernhard von Bülow, who had served as imperial chancellor for nearly a decade. From December 1914 to May 1915 the German statesman, who had many friends in Italy and, incidentally, left us his own interpretation of matters in his controversial memoirs, tried to impress the German viewpoint on the Italians. These negotiations, partially because they were of limited significance to the outcome of the war, have not been at the center of the lively disputes over World War I, especially among German historians, since the early 1960s. Alberto Monticone evidently feels that the talks and their failure reflected the different cultural traditions and the state of public opinion in Germany and Italy. In fact, he would like to see his study inspire a new phase of historical research, one concerned with the interrelations of domestic policy, public opinion, and foreign policy. The author's detailed description of German schemes to buy a sector of the Italian press is interesting and based on a careful analysis of the documents. Whether, however, German interference with Italian public opinion, or with whatever presented itself as that, was a reflection of the domestic situation inside Germany rather than a common procedure in international relations, particularly in time of war (compare this with the activities of Bolo or the German press policy in the U.S. prior to 1917), is quite another question.

Some might be inclined to argue that Monticone takes the entire Bülow mission too seriously because he overestimates the political clout of the former chancellor. He does emphasize the evident differences between the elderly statesman of another era and Bethmann-Hollweg as the representative of the new aggressive Germany, but the reader will wonder whether Berlin's inability successfully to negotiate was not also, in large measure, a consequence of unrealistically high German expectations. In short,

the book is a welcome contribution to the study of a lesser-known phase of the diplomatic haggling that accompanied the contest of weapons and men.

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JOHN H. MORROW, JR. *German Air Power in World War I*. Lincoln: University of Nebraska Press. 1982. Pp. xii, 267. \$21.50.

Readers of John H. Morrow, Jr.'s *German Air Power in World War I* will be disappointed if they expect the standard treatment of the exploits of the famous fighter aces or a highly technical reference work on aircraft statistics and performance. The focus of this scholarly monograph is the history of the German military aircraft industry from mobilization in 1914 through demobilization and the brink of collapse in 1920—a continuation of Morrow's fine 1976 study of the German aircraft industry from its beginning to the outbreak of the First World War (*Building German Airpower, 1909–1914*). Building on the concept of the military-industrial complex, Morrow investigates the civil, military, and industrial aspects of the development of the German aircraft industry to meet the demands of mass warfare. In addition to providing comparisons with Allied aircraft production efforts, Morrow also references developments within the Bavarian air industry, German naval aviation, and Austro-Hungarian military aviation in order to complete the picture of German aviation mobilization.

Morrow's findings, based on extensive archival research and supplemented by a first-rate bibliography, support the view that the airplane was the "most significant new technological weapon" in the German arsenal (besides the U-boat) and was accorded the highest priority in weapons production not simply because of metal shortages, as some have argued, but because the German High Command considered the airplane indispensable to the war effort. In spite of the administrative schisms, inter-service rivalry, difficulties with labor and material procurement, and the inadequate centralization of the German and Austro-Hungarian states, the aircraft industry and the German army succeeded in increasing production from 700 planes in August 1914 to 17,000 planes in 1918; and, more important, given the numerical advantage of the Allies, they succeeded in developing a clearly superior aviation technology. The central role of the army in determining pricing, patent rights, and contracts, and in stimulating innovation resulted in the rationalization of the German aircraft industry—a development that, according to previous historians such as Edward Homze (*Arming the Luftwaffe: The Reich Air Ministry and the German Aircraft Industry, 1918–*

1939 [1976]), did not begin until the 1920s and 1930s.

Morrow's analysis of the postwar disaster of the German aircraft industry also fills an important gap in the history of the German air force largely overlooked by most studies on the development of the *Luftwaffe*. Morrow points out how the Versailles Treaty failed to destroy German military aviation because of the military's determination to rearm secretly, while the restrictions on civil aeronautics forced the few remaining German aircraft industries to collaborate with the military's clandestine rearmament efforts if they were to survive.

As useful as the concept of the military-industrial complex has been for broadening scholarly analysis of military policy and industrial development, it is clear from Morrow's study that the relationship between the military and industry was decidedly one-sided throughout the Weimar period. The army remained the driving, directing force throughout the entire period, maintaining control over the manufacturers and intervening even to the point of bringing collective bargaining to the aircraft industry in order to maintain production. The continuity between German military aviation in World War I and in World War II in terms of tactics, personnel, organization, and economic mobilization deserves closer attention in light of Morrow's research.

The comprehensive scope of this book raises a number of questions regarding specific topics that readers may wish to see addressed in more detail than Morrow allows. The role of personalities and the interrelationship between tactical developments and technology are two topics that made this reviewer curious for more information. Given the problem of the destruction of the *Luftwaffe* archives in World War II and the scattering of surviving records in archives in Bavaria and Vienna and in the German naval archives, Morrow has done an excellent job of pulling together the available sources and data. Future works on the development of German aviation in World War II will clearly have to build on Morrow's foundation.

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HENNING KÖHLER. *Das Ende Preussens in französischer Sicht*. Foreword by OTTO BÜSCH. (Veröffentlichungen der Historischen Kommission zu Berlin, number 53.) New York: Walter de Gruyter. 1982. Pp. x, 122. DM 58.

One of the more fascinating developments in recent German intellectual and cultural life has been the *Preussenwelle*—a spate of books and events commemorating the special Prussian heritage. These

activities, which reached their climax in the spectacular 1981 Prussian Exhibition in Berlin, seem to mirror a reawakening of German national pride in parts of their national heritage repressed after the war under the weight of Western Allied disapproval. Presented with a minimum of fanfare and a maximum of scholarly and international awareness, they surely represent a further positive step of *Vergangenheitsbewältigung*, of accepting and overcoming a difficult past.

A particularly interesting venture of this *Preussenwelle* is the series of studies on the demise of Prussia as seen by other nations, which are being published by the *Historische Kommission zu Berlin* and of which the present little volume is a part. It was prepared by Henning Köhler, a young Berlin historian well equipped for the task by his earlier major study of France's German policy of 1918–19. And he argues persuasively from that perspective that French reactions to the dissolution of Prussia should be most enlightening of all, because France had tangled with the Prussian phenomenon longer and more intensively than any other nation. Yet 1945–47 was not 1918–19. Post-1945 French records are not open and, more important, after the trauma of Nazi German occupation, French political leaders were only marginally concerned with Prussia per se any longer. And worse for Köhler's undertaking, the French press was on strike at the moment of the formal dissolution of Prussia in February 1947 and thus unable to comment. There was, therefore, no public French reaction to the "end" of Prussia. The book, as entitled, could not be written.

Still, Köhler has produced an enlightening and stimulating commentary on the French image of Prussia as it found expression in post-1945 French policy goals for Germany. He was able to show that the almost unanimous triple French demands for German dismemberment or loose confederation, cession of the industrial Rhineland-Westphalian regions, and democratization were frequently clothed in the curiously anachronistic slogan of "deprussianization." In contrast to the historical association of Prussia with the eighteenth-century cultural and political Enlightenment held by the Germans, the French image was that of "blood and iron" Prussian military power in the age of Bismarck and the last Hohenzollerns. Under the influence of a long nationalist tradition dominated by Jacques Bainville's *Histoire de deux peuples* and the *Action Française*, the French regarded German national unity as an artificial creation of Prussian military power. The destruction of Prussia would therefore automatically lead to the re-emergence of the German particularist states as they existed in the seventeenth and eighteenth centuries. Köhler's witnesses for this French perspective range from policy statements of Georges Bidault, Bidault's government

colleagues, Charles de Gaulle, and the Socialists, to parliamentary debates (one reproduced in the appendix), two journals lobbying for the cession of the Rhineland, and the writings of the *Action Française*. In the end the book is unquestionably a condemnation of the French view both as a policy of 1945–47 and as a historical interpretation of Prussia.

DIETHELM PROWE
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GORDON D. DRUMMOND. *The German Social Democrats in Opposition, 1949–1960: The Case Against Rearmament*. Norman: University of Oklahoma Press. 1982. Pp. ix, 374. \$27.50.

A gap has long existed in our understanding of the history of West Germany's postwar integration into the Western alliance, namely, a detailed account of the Social Democratic party's (SPD) struggle against German rearmament in the 1950s. Gordon D. Drummond's scholarly treatise now closes this gap with his detailed account of intraparty and external maneuvering of the SPD under Kurt Schumacher and Erich Ollenhauer. In this book a tale unfolds of bright pacifistic hopes tilting against a world of unpleasant political reality based on power, with rare SPD leaders daring to point out the mismatch of policy and circumstances. The author is at his best when he describes the basic disagreements between the pacifist and even neutralist SPD membership and their timid, if more realistic, leaders. Leaders like Schumacher, in any case, had their own share of contradictory and ill-conceived approaches. The book also has its weaknesses, of course, beginning with the inaccurate title: the opposition stance outside of defense and foreign policy is clearly under-represented, even though one cannot help thinking that it must have been the primacy of domestic policies on SPD minds that kept them from clearly recognizing the international situation of postwar Germany. There are occasional lapses with German words, such as "*Arbeiterpartei*" (p. 4), and possibly the least useful index this reviewer ever tried to use. It is also easy to quibble about a number of dubious statements in the introduction: when Drummond first accounts for the years of the SPD in opposition, for example, he has forgotten the Hermann Müller cabinet of 1928–30, although, in a manner of speaking, perhaps the SPD was indeed already striking an oppositional stance even under a chancellor of its own. The references to SPD defense policy under the Weimar Republic are rather misleading and incomplete. And it hardly seems fair to characterize the SPD party conventions as "propaganda shows where delegates adopt resolutions drafted by the leaders" (p. 5), for this characterization is not borne out by the author's own descrip-

tions later in the book. There is, however, a good chapter of conclusions to tie together the decade covered by the book.

Drummond's portrait of the controversial Schumacher is memorable, although the author downplays the deep-seated nationalism of the SPD leader who, more than any other German postwar figure, symbolized the injured German World War I soldier, with an anti-Russian phobia worthy of a border-dwelling German and anti-French feelings hardly less antagonistic. The description of Schumacher's charisma and boundless arrogance and the pride of a man and a party that, unlike their adversaries, felt no sense of guilt about the recent German past are also very good. The reader might have hoped for more emphasis on Konrad Adenauer's rejection of German nationalism out of this same sense of guilt and his realization that Germans would have to go to extraordinary lengths of cooperation with the West to live down that past. It might also have been good to pay more attention to SPD opinion and initiatives at the state and local levels to describe the life of the party during this long, dry stretch out of power in Bonn. Other smaller desiderata are: (1) some concrete information on SPD views regarding the reintegration of old Nazi and Waffen SS officers, rather than just vague ideological statements; (2) some expression of SPD opinion on the Soviet invasion of Hungary in 1956; and (3) information about the scientific character of the "poll taken by a Frankfurt newspaper" in 1950 (p. 54). Still, when everything has been weighed, this book is a valuable contribution to a difficult subject. Its value is enhanced even more when we think of the current reversion of SPD opinion to the pacifism and neutralism of the 1950s. The author deserves praise for showing the Brobdingnagian giant, the postwar SPD, tied down with fetters made by its own utopian reservations about the world we live in until it finally rose to its full height in the decades that followed.

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PAOLO PRODI. *Il sovrano pontefice: Un corpo e due anime; La monarchia papale nella prima età moderna*. (Annali dell'Istituto Storico Italo-Germanico, number 3.) Bologna: Il Mulino. 1982. Pp. 422. L. 25,000.

Sprawling in the time span and phenomena it attempts to interpret and sometimes impressionistic in the documentation it adduces, this book is nonetheless a landmark in the study of the modern papacy and its relationship to the political development of early modern Europe. Paolo Prodi is best known for his two volumes (1959, 1967) on Gabriele

Paleotti, bishop of Bologna in the period just after the Council of Trent. The present volume, in contrast to that well-controlled study, is a wide-ranging essay. In it the author, fully aware of the difficulties involved, professedly moves into a vast and uncharted territory to try to trace a first map (p. 7) and to break the "conspiracy of silence" (pp. 15, 167) about the role of the papacy in the formation of "the modern state." He wants, above all, to break the historiographical tradition that views the papacy of this era almost exclusively in terms of its relationship to the problems of religious reform and the Reformation. One of the great merits of the book, in fact, is the running commentary it provides on the historiography of political and ecclesiastical structures in the sixteenth and seventeenth centuries.

The book advances a number of broad theses, most of which confound our conventional generalizations about the modern papacy. Historians have tended to characterize that papacy as simply undergoing certain "reforms" while fundamentally continuing the institutions and religious policies of earlier times. It was thus a negligible factor in the development of the modern state. The fundamental thesis underlying Prodi's book is, as I understand it, that under deceptively similar formalities the papacy underwent a deep metamorphosis and that this metamorphosis promoted and exemplified like changes in the political physiognomy of Europe. While worth studying for its own sake, the modern papacy is a key as well to understanding other phenomena. In an age when royal absolutism advanced under the impetus of the powerful alliance of throne and altar, the papacy offered a first model of the modern priest-king.

The transition from "the lands of St. Peter" to the Papal State during this period is the focus of the book. This transition introduced into papal policy and ideology a new schizophrenia—"two souls"—that led, according to Prodi, to a "secularization of the church and a clericalization of the state" (p. 353). Prodi follows the development of this change through the eight chapters of the book, each of which details the relationship and reciprocal influence that secular and religious factors had on each other. The range of documentation and the breadth of viewpoint are almost overwhelmingly impressive, but, fine historian that he is, Prodi always brings the reader back to the fundamental points he is scoring.

By now it should be clear that this book will be an essential component in any future discussions of both the "Counter Reformation" and the development of modern political structures. It definitively breaks the image of simply a "reformed papacy"—of an institution still of some interest to historians of religion but practically irrelevant for the story of early modern Europe. A changed papacy it surely

was—how "reformed" is quite another question. The impact of that changed papacy on political institutions outside the Papal State seems to me more problematic, but I will leave assessment of that aspect of Prodi's achievement to others more qualified to judge than I.

JOHN W. O'MALLEY
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FEDERICA AMBROSINI. *Paesi e mari ignoti: America e colonialismo europeo nella cultura veneziana, secoli XVI–XVII*. (Deputazione di Storia Patria per le Venezie, Miscellanea di Studi e Memorie, number 20.) Venice: Deputazione. 1982. Pp. 308.

Readers will pick up Federica Ambrosini's survey of Venetian encounters with sixteenth- and seventeenth-century writings on European colonization of the Americas expecting to find there a peculiarly Venetian response. They will learn that Venetian readers put into their libraries a rich literature on Spain's colonial policies, which offered sharply contrasting views of the Spanish mission and its impact on native peoples. They will find a frequently reiterated judgment that Venice's patricians were too fixed in their outdated provincialism to appreciate the widened world's significance, particularly that of the "little Englands" taking root in North America. They will learn about attempts to update the Venetian myth by extolling the marvels and piety of Venice as more worthy objects of fascination than rude America. They will be stimulated by a number of intelligent suggestions: for example, that concern with Spanish power colored Venetians' attitudes toward other countries' overseas activities. But they will not find these strands woven into an integrated picture of the reception that Venetian society gave the news of the New World.

The book's greatest success is its cataloguing of the many sides of Spanish colonial activity and the literature on them that found its way onto Venetian bookshelves. Spain's role as the great Christianizing nation, its brutality toward its Amerindian subjects, and the satisfaction that anti-Habsburg Venetians of the Interdict period took in its beard-singeing by English freebooters are all interestingly treated by Ambrosini. Her searching discussions of political, racial, and even theological ingredients in what Venetians could read, and sometimes wrote, about Spanish colonization are especially rewarding. Less persuasive, however, are the many pages Ambrosini devotes to English colonization, which she repeatedly faults the Venetians for ignoring in their unwaveringly "hispanocentric" concern with European power relations (p. 216). Since her main sources in this section are dispatches by Venetian ambassadors in London, we perhaps should not be surprised that

they deal more with politics and diplomacy than with life in the colonies.

The misfit here between sources and conclusions illustrates the book's major weakness, its failure to trace the impact of geographical and politicoreligious writings on the Venetian reading public. Ambrosini has lots of company in scorning the seventeenth-century patriciate's politics and culture, but diplomats' attention to immediate political concerns is not persuasive evidence of a class's flagging interest in the world around it. *Paesi e mari ignoti* is a thoughtful consideration of European literature on the New World, and it fleshes out some of the ideas in J. H. Elliott's *The Old World and the New* (1970). But by offering little documentation or analysis of the Venetians who read about it and their responses to it, Federica Ambrosini fails to achieve her goal of showing how America penetrated the idiosyncratic culture of early modern Venice.

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STEPHEN TOBRINER. *The Genesis of Noto: An Eighteenth-Century Sicilian City*. Berkeley and Los Angeles: University of California Press. 1982. Pp. 252. \$95.00.

In 1693 earthquakes destroyed the Sicilian hill town of Noto, located thirty-two kilometers southwest of Syracuse, forcing the population to abandon forever its ancient patrimony. The Netinese resettled in the rolling countryside near the Ionian coast, where they rebuilt the town on a spacious grid plan, erecting houses, palaces, and churches in the Late Baroque style then prevalent on the island. Having escaped further major natural disasters, as well as the disruptions accompanying modern industrial development, Noto exists today as an almost perfectly preserved eighteenth-century urban environment.

Stephen Tobriner's book participates in the general revival of interest in eighteenth-century urban planning and architecture, and it joins the growing ranks of focused studies of provincial centers in Italy. Thoroughly acquainted with Noto itself and with the local literature, Tobriner provides a stimulating reassessment of the town in this first monographic study to appear in English. As an architectural historian, Tobriner is primarily concerned with the layout of Noto and with the strikingly homogeneous style of its buildings. Those seeking a detailed account of Noto's social, political, and economic history will not find it here.

The richly illustrated book is divided into five parts. In the first Tobriner furnishes a succinct portrait of Noto Antica—a densely built, "organic,"

medieval town of about 12,000 people—and then reviews the political vicissitudes of selecting a new site. As disease, fire, and emigration had reduced the population to one-third of its former strength, the nobility, the lower classes, the clergy, and agents of the Spanish crown disputed whether to rebuild or to refound Noto, a question that eventually was decided by the Spanish authorities in favor of resettlement.

The study's second part is an examination of Noto's early planning history. Utilizing chronicles, descriptions, and a plan drawn by the engineer Giuseppe Formenti in 1702—a plan that unfortunately is not illustrated—Tobriner presents a graphic reconstruction of the layout in 1712. The plan is distinguished by the existence of two imperfectly aligned grids, a fact that Tobriner explains as the result of two successive campaigns, in 1693 and 1694 by the Viceregal Commissary General Don Giuseppe Asmundo. Tobriner also would have the Jesuit engineer Fra Angelo Italia as the chief designer on the basis of a few formal similarities to Italia's nearby town of Avola. The plan of Avola, however, was derived from schemes published by Italian architects in the sixteenth and early seventeenth centuries, and these were available to any architect. It is always tempting to view towns like this as if they were artifacts, works devised by single master planners following specific, formal principles. Tobriner is aware that such an interpretation is difficult to maintain at Noto, but his argument is weakly presented. After carelessly asserting that Noto "was planned to be viewed from east to west" (p. 76), he discounts the supposition that such an idea was part of the initial scheme. His own careful research musters evidence establishing that Noto's spectacular layout was spawned gradually by government and church officials, independent engineers, and the Netinese themselves rather than by a brilliant, definitive master plan.

Part 3 is an insightful discussion of building types. Ecclesiastical structures, palaces, and more modest dwellings are described and fully illustrated. Tobriner perceives a basic duality between the ambitious modern facades and the relatively plain spaces behind them, and he points out that within the orderly blocks of the plan the lower classes developed their habitations in an "organic" or naturally irregular fashion.

The fourth part focuses on three leading architects working in Noto: Rosario Gagliardi, Vincenzo Sinatra, and Paolo Labisi. Here Tobriner enlists an ample selection of fresh illustrations of plans and views of the major buildings in order to set the unique architectural style of Noto within the wider context of the European Baroque. Thanks to books, engravings, and occasional trips away from remote Noto, the architects were abreast of progressive

developments elsewhere but felt free to pursue their own imaginations with visually stunning results.

The conclusion offers an overview of Noto's survival, frozen in time, and is followed by a detailed scholarly apparatus of notes, seventeen pages of selected documents, and a bibliography. The volume exemplifies a new current within architectural history in its treatment of city planning along with discussions of individual buildings and architects. As such it will be a model for future studies of new towns of many periods and places. For Noto and Sicily, meanwhile this is fundamental reading.

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ALEXANDER DE GRAND. *Italian Fascism: Its Origins and Development*. Lincoln: University of Nebraska Press. 1982. Pp. xii, 174. Cloth \$16.50, paper \$7.95.

Slender in size but not in substance, this history of Italian fascism by Alexander De Grand commends itself for its clarity, compactness, and grasp of the subject. It manages to deliver a great deal of information within an interpretative framework that makes it possible for the author to express definite judgments on the nature of the Fascist movement and regime. Following an approach largely inspired by Renzo De Felice's work on Mussolini, De Grand distinguishes between movement and regime, seeing the former as the carrier of conflicting aspirations and tendencies, which, in turn, conditioned the politics and performance of the regime. The discussion of the political decision-making process is one of the strong points of this study. Another is the attention it pays to educational policies and problems of mass mobilization. The introduction clearly states the author's view that, "however complicated the distribution of power within the regime, fascism was clearly dominated by conservative social and economic forces. . . . Fascism in Italy was a rightist approach to the problem of controlled change in an era of mass politics" (p. xii).

Perhaps surprising, given De Grand's previous work on Giuseppe Bottai and the nationalist ideologues of the regime, but fully justified in the opinion of this reviewer, is the consideration of ideology as a secondary aspect of Italian fascism. The only sustained discussion of ideology occurs near the end of the book in a separate chapter entitled "The Political Culture of Fascism: Ideologies and Intellectuals." Here the author concludes: "Fascist cultural policy was the inevitable result of the method of rule which Mussolini applied to politics and economics. The regime could allow no single ideological or cultural perspective to emerge. An unintended side benefit of this confusion was that the Fascist regime did not disrupt the

continuity of Italian culture to the extent that the Nazis did in Germany" (pp. 152–53). It is an assessment that underscores the failure of Italian fascism to establish a full-fledged system of totalitarian control and acknowledges that considerations of political expediency motivated the use of power. Valid as this assessment seems to be for the internal history of the regime, it may not provide an entirely convincing explanation of Fascist foreign policy. The expansionist foreign policy appears here entirely as an expression of Mussolini's will to dominance—a view that skirts the debate over the relative weight of ideological versus power-politics motivations.

To observe that most of the material presented here will be familiar to specialists is not to criticize the book. It should be welcome wherever there is a need for a reliable, concise, and readable account of the Fascist experience. There are, of course, assertions that one can quibble over. To describe the March on Rome as merely "an exercise in psychological warfare" (p. 36) is to overlook that it was accompanied by Fascist takeovers in many provinces; that Mussolini was interested only in normalization after the March on Rome is a view contradicted by some of the Duce's own statements at the time. Neither the discussion of the corporative reform of 1934 nor that of Mussolini's and Alberto Beneduce's expectations in founding the Institute for Industrial Reconstruction is entirely satisfactory. But these are minor reservations that should not discourage anyone who might want to adopt this book for classroom use or recommend it to readers who need a concise and readable account.

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FIKRET ADANIR. *Die Makedonische Frage: Ihre Entstehung und Entwicklung bis 1908*. (Frankfurter Historische Abhandlungen, number 20.) Wiesbaden: Franz Steiner. 1979. Pp. xi, 283. DM 64.

Macedonia was the part of the Balkan Peninsula that the Ottoman Turks occupied first and evacuated last. Until the 1950s many important Turkish officials were of Macedonian background. It is thus fitting that Fikret Adanir, a Turkish historian based in West Germany, should tackle the thorny Macedonian question in European history. Adanir ends his investigation in 1908, the year of the Young Turk revolution, but the Macedonian question was not resolved then or even four years later when the Ottomans were compelled to leave the region. It continues to complicate inter-Balkan relations to this day.

An expanded version of his 1977 doctoral disser-

tation at the Goethe University in Frankfurt, Adanir's book disputes the "national-historical" perspectives of much Balkan historiography. The author contends that historians cannot understand or explain Macedonia's turbulent past by focusing on a single revolutionary movement or resorting to time-worn assertions of class conflict between Ottoman "exploiters" and Macedonia "exploited." Rather, scholars must examine a much broader Macedonian question that reflects the interplay of indigenous ethnic concerns with a panoply of partially independent social and economic factors and with external forces such as inter-Balkan national strivings and the imperial interests of various European powers.

Adanir begins with a broad picture of political, social, and economic developments in the Ottoman empire over several centuries. He closely examines the Ottoman *millet* system that provided the framework within which a separate Slavic-Macedonian identity gradually arose, one greatly influenced by outside forces. In his second section, he looks at the "preparatory phase" of the Macedonian revolutionary movement (1880–1902), expertly tracing the development of the Macedonian Revolutionary Organization of 1893 into the more familiar IMRO and discussing the extensive cultural warfare waged by Bulgaria, Greece, and Serbia in Macedonia during the period; the effects of Bulgarian–Eastern Rumelian union on Macedonian affairs; the introduction of *četa* guerrilla fighting; and the Džumaja insurrection of 1902. A short third section considers the numerous revolutionary activities of 1903, including the Gemidžii-inspired Salonica uprising in April and the famous St. Elijah's Day (Ilinden) uprising of midsummer. The final section deals with the five years after Ilinden, culminating in the Young Turk revolution, when the Macedonian question entered a new phase.

Adanir writes in lucid German and cites a vast array of primary and secondary sources in many languages, including Turkish. He makes good use of diplomatic materials from the Haus-, Hof-, and Staatsarchiv in Vienna and the Politisches Archiv des Auswärtigen Amtes in Bonn. He deftly leads the reader through the maze of personalities, organizations, religious and national groups, and shifting alliances that characterized the Macedonian question before 1908. Unfortunately, this otherwise impressive work lacks an index, and its unclear map has the name "Bulgaria" encroaching on territory occupied by Serbia after 1878.

Adanir tends to side with Bulgarian historians in their dispute with Yugoslavs on the question of when the Macedonian-Adrianople Revolutionary Organization dropped the attribute "Bulgarian" from its name, but Bulgarians will find little else to comfort them in this book. In fact, Adanir's study will not win many friends in Sofia, Belgrade, Skopje,

or Athens. Many historians will contest his general treatment of the Macedonian question or his gentle treatment of Sultan Abdulhamid II. Others will take exception to his portrayal of the Ottoman empire as the savior of Orthodox Christianity in the Balkans. Always thoughtful, occasionally provocative, Adanir's book is an important contribution deserving of the prize awarded it by Munich's Südosteuropa-Gesellschaft.

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WOLFGANG KESSLER. *Politik, Kultur und Gesellschaft in Kroatien und Slawonien in der ersten Hälfte des 19. Jahrhunderts: Historiographie und Grundlagen.* (Südosteuropäische Arbeiten, number 77.) Munich: R. Oldenbourg, for the Südost-Institut, Munich. 1981. Pp. 352. DM 72.

Conceived as a "preliminary study . . . for a critical history of Croatia and Slavonia" (p. 18) between 1790 and 1848, Wolfgang Kessler's *Politik, Kultur und Gesellschaft in Kroatien und Slawonien* is an excellent work on the history of an area and an era largely ignored in Western-language scholarship. Kessler's main contribution is to provide for this subject a conceptual framework that avoids the conscious or unconscious political motivations of existing Croatian and Yugoslav interpretations.

As a preliminary study, the book does not offer a continuous narrative. The bulk of the work surveys existing historiography, defines terminology, clarifies factual misconceptions, and sketches in the linguistic, religious, cultural, political, social, and economic background of the period. Throughout, Kessler strives to avoid an interpretation that posits the existence of a Croatian nationality before the appearance of the Illyrian movement in the 1830s. He, in fact, concludes that it is wrong to speak even of the much-discussed Illyrian movement as representing full-blown nationalism, since "before 1848 in Croatia and Slavonia practically all the preconditions of a 'modern' nationalism were lacking" (p. 268). At best, the Illyrians represented a national unification movement, composed largely of intellectuals, dependent on public and financial support from the nobility, and cut off from the bulk of the population, the peasantry who composed the Illyrians' purported "nation." Only their successors created a modern nation encompassing all strata of the population. The Illyrians *were* responsible for introducing the principle of nationality into practical politics in Croatia, but their innovative role, Kessler argues, was more noticeable in the cultural than in the political field.

The plausibility of Kessler's depiction of the Illyrians depends to a great extent on his treatment of

Croatian history in general between 1790 and 1848. Consciously eschewing the previous approach of scholarship, which tended to consider the Illyrian movement in a vacuum, Kessler illuminates its nature and goals by describing its political, cultural, and socioeconomic background. In politics, dominated throughout the period by noble interests and activities, the Illyrians' role was minor. After their appearance in the 1830s, they merely buttressed with new arguments the existing noble opposition to Magyar attempts to restrict the limited political autonomy of the Croatian noble estate. In the cultural sphere, their notion of an Illyrian nation and language faced the opposition of numerous competing ideologies among—existing or soon-to-be—Magyars, Serbs, and Slovenes, and even among the future Croats, divided as the latter were among various dialect groups, each possessing some sort of written “language.” (In this regard, Kessler’s treatment of Serbo-Croatian dialects and their literary expressions is especially good.) Finally, in the socioeconomic sphere, the educated Illyrians were part of the tiny minority of the population who lived in the small, multilingual, preindustrial cities: a milieu alien to that of the enserfed, illiterate peasant majority. Yet, if the Illyrians in Kessler’s account seem to be less than the major figures they have been portrayed as in previous historiography, their abortive program for an Illyrian nation and language still was significant for the future course of Croatian history: it “potentially made possible . . . the integration of population groups” (p. 283) into a nation wider in concept than that of the politically enfranchised nobility before 1848.

Kessler’s book on pre-1848 Croatia will undoubtedly be welcomed by scholars of Croatian and Yugoslav history writing in Western languages. It is another question, however, how scholars in and from Yugoslavia will react to this reinterpretation of a major era in their history.

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NORBERT ENGLISCH. *Braunkohlenbergbau und Arbeiterbewegung: Ein Beitrag zur Bergarbeitervolkskunde im nordwestböhmisches Braunkohlenrevier bis zum Ende der österreichisch-ungarischen Monarchie.* (Veröffentlichungen des Collegium Carolinum, number 41.) Munich: R. Oldenbourg, 1982. Pp. 336.

This volume is a welcome addition to our growing knowledge about the Industrial Revolution and its impact on certain local areas in Europe. Norbert Englisch’s study focuses on one of Europe’s great coal-producing areas, northwest Bohemia. By the time he intercepts the story of this region and its people, northwest Bohemia has become the third

richest source of coal on the entire continent. While it is true that the soft coal mined here was geologically less desirable than that found in northern England or the Ruhr, still its social and economic impact was no less startling. Englisch is right to stress that the initial industrial growth of Saxony, eastern Prussia, Bohemia, and the Vienna Basin was all intimately tied to the development of these fields.

This monograph is not, however, primarily concerned with the economic history of the area bounded by the old German cities of Komotau, Brück, and Teplitz but with social considerations. The workers who began to collect in these mining areas were largely drawn from local peasants and artisans. The miners of northwest Bohemia, as Englisch tells us, never really benefited in an outstanding way from the improvements that this new industrial age had to offer. They gained concessions from the Austrian government and from their employers rather slowly. The twelve-hour day was not won until 1884 and the ten-hour day was not common until the late 1890s. These soft-coal miners had Sunday off, but they still worked on Saturday right into the 1920s. Not only this, but the continuous manual effort required of these men was physically exhausting. Their level of energy might have been higher except, as Englisch demonstrates in some of his more fascinating pages, that their wages barely kept up with prices. Truly one of the least rewarded groups of the age, family income up to 1914 was just enough to pay for the staples of their existence—bread, potatoes, and coffee. They did smoke tobacco, but luxury foods such as meats and vegetables were out of their reach.

Conscientious attempts to organize the miners of northwest Bohemia were rarely successful. Long before unions were legalized within the Austrian empire, miners did have their own guild-like organization. These organizations did not, however, readily give way to modern unions. Throughout the 1890s and early 1900s, the soft-coal miners of this area shied away from union activity. Even though a centralized organization existed from 1894, mine-union locals were numerically small, enrolling only a tiny portion of the combined German and Czech labor force. The only time when union leaders attracted the majority to the locals was during the strikes of 1900 and 1912, when nonunionized miners did willingly look to the unions for guidance.

Englich’s intriguing study is a solid piece of scholarship and explanation right up to his concluding remarks. But here he tries to convince his readers that the weary miners of northwest Bohemia were ideologically oriented and inspired by such words as “socialism” and “internationalism.” His own evidence disputes this, for he himself proves all along that these miners were conservative and traditional folks struggling for step-by-step improve-

ments in their lives. Apart from this misunderstanding, however, Englisch has produced an extremely valuable book: a contribution to our coming closer and closer to what actually happened during the Industrial Revolution.

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ALICE-CATHERINE CARLS-MAIRE. *La Ville Libre de Dantzig en crise ouverte 24.10.1938–1.9.1939: Crise locale et crise européenne*. (Gdańskie Towarzystwo Naukowe, Wydział i Nauk Społecznych i Humanistycznych, seria monografii, number 79.) Wrocław: Zakład Narodowy Imienia Ossolińskich, Wydawnictwo Polskiej Akademii Nauk. 1982. Pp. 227.

Alice-Catherine Carls-Maire presents the internal and external aspects of the Danzig crisis in the last ten months of peace and the interconnection between them. She divides her book into four parts dealing with diplomacy (Polish-German relations), political action in Danzig, the anti-Polish policy of the Danzig Senate, and the arming of the Free City in the summer of 1939. Her detailed account is based on an impressive array of Polish, Danzig, French, British, and German archival sources, as well as on published documents and monograph studies.

Carls-Maire fills in many gaps in our knowledge of the internal Danzig situation, which is her main interest and which has been sadly neglected in previous studies of this period. She describes the personal rivalry between Gauleiter Forster and Senate President Arthur Greiser and its effect on the political scene. It was not so clear to contemporaries that Greiser's "moderation" was largely the result of personal rivalry with Forster and that, in fact, the successes and failures of either depended ultimately on the current policy of Berlin toward Warsaw.

Carls-Maire gives us an excellent picture of the mounting tension inside the city, particularly in the summer of 1939 when it became an armed camp. This fact was duly reported by British Consul Gerald Shepherd who was, however, replaced in July by his more phlegmatic namesake, Francis Michie Shepherd. The Polish government kept its nerve in the face of constant provocation—a fact noted and admired even in London.

In the first chapter, which deals with the diplomatic aspect, one would wish for a more succinct account of British and French policy; also, some statements on Polish policy are misleading. There was no risk of Beck losing his reputation by ceding Danzig (p. 17), since he never contemplated doing so. It was not only the military that supported his

policy of good relations with Germany; the opposition parties also did so after Hitler's occupation of the Rhineland in March 1936. It is not clear whom Carls-Maire has in mind when stating that the "Left" favored territorial concessions to Germany. The Socialist party never did, while the Communist party gave up this position in 1933 and was, in any case, dissolved by the Comintern in summer 1938. Burckhardt was wrong to think that Beck was taken in by Ribbentrop's claim that Danzig was of secondary importance in Hitler's overall policy. Beck in fact said in November 1937 that he expected the matter to come up soon; but he did not expect Hitler to make it a pretext for war. Kennard's report of March 29, 1939, that Poland would fight over Danzig should not be read as deliberate disinformation designed to secure British support for Poland (p. 36). In fact, the Foreign Office had accepted this Polish stand as a fact of life for many years before 1939. This was the reason why the British government tried so hard to arrange Polish-German negotiations.

While there may be some disagreements with Carls-Maire's interpretation of diplomatic history and while there is some overlapping in the chapters dealing with internal history, it is high time that attention was devoted to the internal political situation in Danzig in 1938–39. Carls-Maire's study should be read by all those interested in the approach of the Second World War, particularly those who still have a lingering suspicion that Danzig was, per se, the cause of war, or that war could have been avoided by a negotiated settlement fair to both sides. The publication of this study in elegant French will make it accessible to many more readers than would have been the case if it had been published in Polish. Let us hope that it will be as widely read as it deserves.

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JOANNA K. M. HANSON. *The Civilian Population and the Warsaw Uprising of 1944*. New York: Cambridge University Press. 1982. Pp. xiii, 345. \$34.50.

Among the many studies of the Warsaw Uprising in 1944, this book by Joanna K. M. Hanson is a special one offering an assessment of the conditions and behavior of the civil population during the sixty-three days of fighting.

"Grand reportages" and many books and studies of this uprising have been published before this book, but this is the first major history of the uprising that concentrates on the people, on the civilian population. It is based on broad sources, Polish and foreign, on interviews with witnesses, and

on descriptions of major actions of the uprising. It describes a confrontation of the helpless, although heroic, population that desired freedom and independence from the Nazi occupation, with the overwhelming power of the Nazi war machine. The book is written with irreproachable scholarly methods, and yet it is exciting and keeps the attention of the reader.

The study starts with a concise background of the uprising and a characterization of the Nazi regime in Poland and specifically in Warsaw. A detailed, dramatic unfolding of the uprising itself, hour after hour, day after day, for an unbelievable sixty-three days of holocaust, includes a characterization of Polish organization and administration in all districts of embattled Warsaw. Attention is paid to public services, supplies, health service and sanitation, press, entertainment, schooling, involvement in the continuous military fighting and defenses, hopes, and desperation. The place of religion is incisively assessed. Examples of audacity and heroism, sufferings, tears, determination in fighting and living, and sacrifices freely accepted and imposed by overwhelming circumstances are described with sensitivity, fairness, and subtle and sharp perception by the well-informed author.

The dramatic fate of the Warsaw uprising has been put in relief by describing the stand of two big powers: Nazi Germany and the Soviet Union. Himmler's and Hitler's plan to be applied to crush the uprising ordered the total destruction and erasure of Warsaw; men, women, and children were to be massacred; the survivors were to be dispersed among concentration camps and then exterminated (p. 83). Special formations, sadistic criminals of various nationalities, commanded by the SS were then thrown against Warsaw. During the first days of the uprising, forty to fifty thousand people were killed in "Dantean scenes of orgy, bestiality and systematic destruction." Men and women, adults and children, sick and healthy, clergy, sisters, and lay were slaughtered when they already were overwhelmed (pp. 84–85). The casualties grew to nearly two hundred fifty thousand by the end of the uprising.

On the other side, Stalin ordered Soviet radio to broadcast at the end of July 1944 that the Red Army was approaching Warsaw and would help the population if they raised arms against the German occupiers. When the uprising broke out on August 1, the Red Army was stopped at the Vistula River, at the suburbs of Warsaw, on the personal orders of Stalin. He first refused to admit that the uprising had started, then tried to belittle it, then did not permit allied aircraft to land on Polish territory occupied by the Red Army (to the east of the Vistula River) for the refueling necessary for bringing supplies from the West to the insurgents. A few Soviet air drops at

the end of the uprising had only propaganda objectives and disguised the reality.

The book successfully concentrates on the study of the civilian population. It is, of course, extremely difficult to make a strict distinction between civilian and military groups and functions. Therefore, perhaps, some matters of concern for both, such as weapons and ammunition, are not covered adequately. The author could expand her characterization of the *Powisłe* district and of "Unia," a Catholic underground military-political organization, attached to the SP (*Stronnictwo Pracy* or Christian Democratic Labor party). This organization had a small, underground factory for weapons and ammunition. It was directed by Captain Cyprian Odorkiewicz, "Krybar," who had been an assistant to President Stefan Starzynski for one district of Warsaw in free Poland and was one of the commanders of the uprising in *Powisłe*.

This is a carefully edited publication with a few well-selected illustrations, a comprehensive bibliography, careful notes, a list of abbreviations, and an index. It is based on a PhD dissertation obviously written in an excellent doctoral seminar. The conclusion of the book relates issues and reflections to the political reality of Poland that developed after World War II.

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RICHARD HELLIE. *Slavery in Russia, 1450–1725*. Chicago: University of Chicago Press. 1982. Pp. xix, 776.

This is a very detailed study of slavery in Russia, a phenomenon that embraced perhaps 10 percent or so of the population. It is based on 2,499 documents referring to 5,575 slaves, of whom the vast majority (4,009) were contract slaves (*kabal'nye ljudi*); "full" or hereditary slaves (671), a similar group, were the next largest category. The data is somewhat heaped: 92 percent of all the cases are from the northwest (Novgorod) region, and 80 percent are from the years 1581 to 1603. But this is, for Russia, a remarkable set of data, and Richard Hellie has exploited the statistics exhaustively.

The book is divided into two unequal sections: the first and shorter one (about a third of the whole) deals with the law, the second with the sociology of slavery. Eight maps, ten figures, and 112 tables supplement the text. The material is illustrated by accounts of law cases interestingly supplemented by details of the careers of those servitors involved. In addition, the Russian material is augmented by matter drawn from other societies that had slaves, ranging from ancient Mesopotamia to the American

South. The treatment of the Russian data is such that it imposes some demands, both linguistic and statistical, on the reader. A glossary of Russian terms would have made the work more readily available to the nonspecialist (the index fails to perform this function). At times, too, the statistical tables give the impression that calculation has become an end in itself, rather than a tool essential to handle the material. Nevertheless, this is a book rich in material for Russian specialists and those concerned with the comparative study of slavery.

Hellie describes the Muscovite system of slavery as a relatively mild one compared with some other societies; the brutal savagery toward dependents came with the age of Enlightenment. This mildness he ascribes mainly to two facts: the existence of a social scale of slaves that extended from lowly household slaves up to high state officials, and the fact that the majority of slaves were Orthodox Russians. Slave numbers were maintained mainly by natives selling themselves into slavery, "the only kind of welfare relief that early modern Russia offered" (p. 716). But slaveowning was also a prestige symbol for the elite. Slaves apparently conformed more to the European marriage pattern than was formerly thought. Full slavery was superseded by contract slavery, which was at first limited to one year, but, then, if the payment made to the slave at the time of the contract was not cleared, became hereditary. Many slaves, however, fled. It seems to have been assumed both that slaves would flee and that they would again sell themselves. Yet prices remained low, little more than the cost of annual maintenance. After 1586–97 contract slavery was limited to the life of the owner. Almost a century later agricultural slavery, and by 1723 household slavery, were eliminated, and serfdom became increasingly oppressive.

Slave ownership seems to have been concentrated in the upper levels of the servitor class and, geographically, focused especially on Moscow. Although all slaves probably outnumbered other categories in the army, and amounted to perhaps a third of combat troops, many servicemen had no slaves. Hellie argues that slaves in Russia were predominantly household servants and were not much engaged in production. Nevertheless, aspects of the administration in general and of estates in particular seem to have relied a good deal on literate and, presumably, numerate slaves. The range of trades recorded (pp. 488–89) may indicate something of the production functions of great households. The use of slaves by merchants is also of interest. The charter evidence, moreover, seems to suggest that lordly colonization and the relatively restricted working of demesnes were, in an appreciable number of cases, effected by slave labor. None of this is precisely quantifiable, but taken together it seems to

imply that slaves indeed had important, perhaps crucial, productive and administrative functions at levels above that of the simple, undifferentiated households of peasants or lord. Even if we knew the numbers of military slaves as a proportion of the total, this would not of itself tell us whether slaves were important in production. In some ways, on present evidence, slaves might be regarded virtually as a relatively small "hired labor" group in a weakly monetized economy. Early Russia had difficulty in distinguishing hire from servile dependence. Military slaves, like military servitors, were no doubt underemployed and also nonproductive, but they created a demand that itself acted as a stimulus to production.

A book on this scale will almost inevitably evoke queries and some disagreements. Did Russians have a simple conception of slavery or a series of categories of "people" (*liudi*) broadly distinguished from "peasants" (that is, serfs)? How crucial was the concept of service, with its own system of honor, for Russians? How did this shape the attitudes to and of slaves? The whole area of the etymology of terms for slavery, work, and labor seems to call for careful research if we are to make progress in understanding Russian sociological attitudes.

Hellie often presents his material in a modernized form. This, like the inclusion of material from extraneous societies, can sometimes be illuminating, but it runs the risk of violating chronology and sometimes obscuring the main argument. At times, too, it begs too many questions, as in the following quotation: "Full slavery and then limited service contract slavery were stress phenomena, historically an unusual response to the universal human phenomenon of psychophysiological stress. In modern biological parlance, stress distorts the chemical balance of one of the catecholamines, norepinephrine (noradrenalin), and some Muscovites sought relief by selling themselves into slavery" (p. 378).

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L. N. SEMENOVA. *Ocherki istorii byta i kul'turnoi zhizni Rossii (Pervaia polovina XVIII v.)* [Studies in the History of Daily Life and Culture in Russia in the First Half of the Eighteenth Century]. Leningrad: Nauka. 1982. Pp. 279. 1 r.

In the Soviet division of scholarly labors, the history of daily life has usually been handled by ethnographers. With the exception of B. A. Romanov's book published in 1947 on the people and mores of medieval Russia, works of this type by historians have begun to appear only in the past five years. L. N. Semenova's study is the first to treat the eighteenth century. She devotes chapters to the

family, parents and children, fashions and spiritual life, entertainment and leisure, and the material conditions of everyday life and work. She leans heavily on familiar sources like the *Domostroi* (a sixteenth-century guide to household management and personal behavior, whose precepts were still being followed in the early eighteenth century), the *Honest Mirror of Youth*, and the often quoted writings of Ivan Pososhkov, Feofan Prokopovich, Vasilii Tatishchev, and Andrei Bolotov. She also uses many less well-known writings, reports, popular prints, and legal documents, both published and archival, which add interesting and, in some cases, dramatic detail. The only annoyance is her penchant for citing Aleksandr Radishchev, a polemicist writing late in the century; since Radishchev provides the kind of critical comment that is required in Soviet works, the use of this anachronistic and inappropriate source may have been a condition of publication.

Although not a central theme, the history of women much interests Semenova, and she often comments on the place of women in the family and society and changes occurring in women's lives. Western writings on this subject have found a resonance in this work. One section treats the changing image of women. An example of the new perception of their role was Tsar Peter's desire to send young women abroad to study. The proposal, however, met ferocious resistance, and Peter had to settle for occasionally insisting that wives follow husbands on missions abroad. The view of women among the common people may have been reflected in popular prints (*lubki*), which circulated widely and portrayed two contrasting types: the good wife and the tyrannical one (*zlaia zhena*) who forever scolded and even beat her husband. In popular culture, the nasty wife was a frequent subject of morbidly humorous reflection. One print tells of a browbeaten husband who, after the death of his wife, sold his children for fear that they would grow up like their mother and sell him.

About half the section on entertainment is devoted to the political and social significance of the notorious Petrine institution, the "Most Holy Synod of Fools" (*Vseshuteishii sobor*). Semenova disagrees with those who see this institution as a form of recreation. In her view, it reflected existing power relations in the government and society in a vivid and didactic manner. In addition, the noisy and irreverent gatherings at once ridiculed and provided a substitute for the court dinners of Muscovite times. Regrettably, Semenova missed the opportunity to make instructive comparisons—especially regarding the generation from on high of these Russian parodies of power—with Western carnival traditions.

The final chapter, on material conditions, blends observations of contemporaries with statistics on

production of foodstuffs, price movements, taxation, and information about religious fasts. These inquiries lead naturally to ruminations on three other subjects: famine, charity, and alcohol consumption. Semenova confirms that the Russian diet was coarse but adequate. The chief difference between the privileged and the common people was in the quantity of meat and sugar consumed. Semenova offers some complaints on behalf of the less well-to-do, but the sting of these criticisms fades beside evidence that for peasants fish, berries, and honey were often plentiful and provided nutritious substitutes for meat and sugar.

Although it is useful to have these details of everyday life packaged neatly, Semenova's study fails to rise above a collection of facts and observations. She has not found a method that would allow her scattered data to illuminate larger issues of politics and society. Her book is nevertheless a welcome sign of new stirrings in Soviet studies of social history.

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WALTER J. GLEASON. *Moral Idealists, Bureaucracy, and Catherine the Great*. New Brunswick, N.J.: Rutgers University Press. 1981. Pp. ix, 252.

This book by Walter J. Gleason studies the public careers of three men prominent during the reign of Catherine the Great, the journalist Nikolai Novikov, the poet Ippolit Bogdanovich, and the playwright Denis Fonvizin, all of whom at one time worked for Nikita Panin, Catherine's chief minister. It is revisionist in intent, rightfully seeking to refute Soviet historians (and their nineteenth-century liberal predecessors) who cast these intellectuals "in a heroic pose" as serious opponents of Catherine's policies; rather it shows the extent to which they worked within the system as Panin's intellectual and publicistic aides. In this it is eminently successful, focusing as it does on their entire service careers rather than on the one moment when Catherine closed their journals. Others have challenged this old notion of a gentry *frond* in eighteenth-century Russia, and they will gain much by reading this account of the intellectual underpinnings of the Panin party, unearthed in 1975 by David Ransel.

The second purpose of this book is to explore "the legal language in which the monarchs spoke to their own governments and subjects" by determining "how well versed [the Fonvizin group was] in German natural law." In this endeavor Gleason elaborates on an idea proposed by Marc Raeff, that the statist German *Aufklärung* was distinct from the liberal French or British Enlightenment, and more

influential in Eastern Europe. Raeff's revisionism has been one of the most significant new lines of thought in recent eighteenth-century historiography, but unfortunately his case is not helped one tittle by Gleason. Indeed, the theme of German natural law represents the weakest part of the book. First, Gleason's account of the political and social thought of Pufendorf and Wolff is inadequate, consisting of scarcely more than a half-dozen pages. The danger in such brevity is oversimplification. When we learn that the German enlighteners "emphasized each person's duties to his fellow man" (p. 58), believed that "the monarch governed according to 'fundamental laws'" (p. 89), and that the monarch "must avail himself of the counsel of wise and knowledgeable men" (p. 118), we are in danger of diluting a noble intellectual structure to platitudes. As in the case of a historian of education who marveled over an injunction to seek out teachers of high moral character, we are led to ask, whoever would suggest the contrary? The first problem, therefore, is that German natural law is not clearly enough defined so that it can become a reliable measure of Russians' interpretations of it.

When Gleason turns to the fate of German natural law in Russia the results are equally disappointing. A lengthy first chapter on Lomonosov and Sumarokov suggests that although they were exposed to it they ignored, "cared little," for it. Next we are told a great deal about M. M. Kheraskov, the vibrant mentor to the Fonvizin group in their teenage (and subteenage!) years, but nothing of his own education or his teaching at Moscow University. This second chapter treats a series of short-lived journals for which the group wrote and translated between 1759 and 1764, but no German natural law theorist is visible in their pages; instead we learn that Pope's *Essay on Man*, Samuel Clarke's *Proofs of the Existence . . . of God*, and a book of *Contemporary French Philosophy* were translated by the group. This same harsh judgment can be made about the chapters on the coup of 1762, on the early civil service careers of the group under Panin's patronage, on their activities during the sessions of the Great Legislative Commission, on Fonvizin's plays of social commentary, and even on Novikov's directorship of the University Press between 1779 and 1789. Nowhere is there any uncontrovertible evidence that Pufendorf and Wolff were major influences on the thought of the Fonvizin group, to the exclusion of the French and British enlighteners. Indeed, a great deal of contrary evidence exists in the book itself, and had Gleason considered literary influences on Fonvizin and Bogdanovich, or the total corpus of translations published by Novikov, he might find himself forced to abandon the Raeff hypothesis altogether.

In short, this reviewer finds the principal asser-

tion of the book, that Moscow University trained "a small number of men in the specifics of natural law" and that this German training accounts for the content of Russian ethical and political thought in the age of Catherine, to be unconvincing and unsubstantiated. By contrast a subtheme on the role of neo-Stoicism is much better documented: at least the Fonvizin group did translate the Stoics. This is still an important book, which gives us an unprecedented glimpse of an early generation of Russian intellectuals questioning values and seeking answers in Western thought and in the Russian and classical past, but above all, trying to do so within the hostile environment of the Russian service system, the client networks of the court, and within a society often suspicious of even their questions.

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PETER K. CHRISTOFF. *An Introduction to Nineteenth-Century Russian Slavophilism*. Volume 3. *K. S. Aksakov: A Study in Ideas*. Princeton: Princeton University Press. 1982. Pp. ix, 475. \$37.50.

Peter K. Christoff's *K. S. Aksakov: A Study in Ideas* is the third in a four-volume study of nineteenth-century Russian Slavophilism (the first two were on Khomiakov and I. V. Kireevsky). Christoff's work on Aksakov should be read in conjunction with its predecessors. The final volume on Iu. F. Samarin has not yet been published.

Christoff has divided his study of Aksakov into two parts: the first, "The Nobleman's Nest," is concerned with Konstantin Aksakov's upbringing and education at home, his life at the University of Moscow and in the Stankevich Circle, and his relationships with other Slavophiles, Westernizers, and members of the intelligentsia who did not fit into either camp. The second part is a topical examination of Aksakov's thought and is entitled "The Choric Principle and the 'Land.'" It analyzes his shift from Hegelianism to Orthodoxy; his about-face on the question of the significance of Peter the Great; and his attitudes toward the *narod*, commune, choir, *zemsky sobor*, the position of the state in relation to the people, and, finally, toward the impending emancipation of the serfs.

It is one of Christoff's theses that Konstantin Aksakov, like the other three seminal classical Slavophiles, could not be dismissed as simply "conservative," "romantic," and "utopian," as some students of Russian thought have contended and continue to contend. Rather Aksakov's choric principle and his concept of the Russian peasantry were deeply rooted in Russian Orthodoxy, not so much in the sense of a formal hierarchical church but rather in the spirit of communal, Christian, brotherly love.

Western reason, the role of the individual, laissez-faire economics, private property, the state with its bureaucracy, and the burgeoning proletariat and bourgeoisie of the West were viewed as alien to the Russian choric principle. This was a principle that, Christoff believes, was influenced not so much by German idealism and romanticism as by Aksakov's immersion in Russian communal, Orthodox thought, albeit through the prism of the nobleman's nest.

Aksakov's weaknesses, his one-sidedness and his lack of professional training in history, as well as his polemical diatribes and lack of systematic thought, are compensated in Christoff's mind by his Slavophile social consciousness. He even views Aksakov as having this trait in common with the radical Westerner Belinsky. Christoff, however, shows the parting paths of the Slavophile and Belinsky when the latter opted for "positivistic socialism while Aksakov raised the Russian communal principle to a Christian verity" (p. 428).

In conclusion, Christoff has made a great contribution to an understanding of Konstantin Aksakov and his contribution as the "historian" or, better, the "poet-historian" of the Slavophiles. Christoff's treatment has achieved a unique balance between objectivity and a sympathetic understanding of the ideas of Aksakov and his contemporaries.

Christoff's work is enhanced by an outstanding bibliography, index, and a group of carefully selected illustrations.

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JOACHIM KRUMBHOLZ. *Die Elementarbildung in Russland bis zum Jahre 1864: Ein Beitrag zur Entstehung des Volksschulstatuts vom 14. Juli 1864.* (Quellen und Studien zur Geschichte des Östlichen Europa, number 15.) Wiesbaden: Franz Steiner, 1982. Pp. ix, 304.

This narrow but richly detailed study of the evolution of Russian elementary education from its beginnings in the reign of Peter the Great until the promulgation of the elementary school statute of 1864 is a sound and welcome addition to a growing literature on education in imperial Russia. Hitherto, focus has been placed on secondary and higher education, an approach that has a certain logic in view of Russia's educational development from the top down, but this present study examines the evolution of the primary schools and the policies of the state toward them.

A number of important truths emerge from Joachim Krumbholz's survey of this process; for one thing, the government never possessed a coherent or consistent policy to provide a framework within

which to evolve a comprehensive system of primary education, nor did the state succeed over a century and a half in developing a unified or standardized structure. Primary schools were formed within a variety of jurisdictions to meet a variety of needs. Inadequate training and compensation for teachers seriously hampered pedagogical effectiveness, but perhaps the most pervasive problems were the matters of what was to be taught to whom and the coordination of primary schools with other levels of education. It was in such issues as these that education, by enhancing possibilities of social mobility, challenged the status of the gentry class; the mere imparting of literacy raised questions about access to higher education and hence to all of the benefits that might flow from membership in an educated elite. It is not surprising that Krumbholz's analysis of the public debate on education after the death of Nicholas I reveals a spectrum of attitudes that fully accords with views toward such issues as the impending emancipation and other great social questions. Indeed, the author notes that after the emancipation a powerful negative backlash dampened the efforts by the new minister of education, A. V. Golovnin, to deal comprehensively with the deficiencies in the system of elementary education.

Despite the grand sweep of this work, the focus is on the process that produced the elementary school statute of 1864; the author meticulously traces each step within the Ministry of Education and other governmental bodies and interweaves the public debate that accompanied the process. In the end, the resulting statute fell far short of realizing the hopes and justifying the prodigious efforts of its framers; indeed, the author cites a comment to the effect that the mountain had given birth to a mouse. Russian elementary education continued to be chronically underfunded, teacher training was still a problem, school administration was undeveloped, and so on. Many of these were questions to be addressed by the new *zemstva* in future years. In all this, the author is probably less critical of the personal incapacities of Golovnin than he might be; despite the very real impediments to reform, one cannot help but wonder if the result would have been the same if Golovnin had been a person of the stature of a Reutern or a Miliutin.

Krumbholz probably tries to do too much; the study tends to be quite narrow and important educational questions are not always presented within the context of other pertinent currents in state and society. When he does discuss the role of public debate, he does not tend to go beyond the dialogue in the press, and the primary schools situated within other ministries and jurisdictions often appear only when they have some specific importance for the schools of the Ministry of Education. There are also the small errors that inevitably creep into every

work; it was amusing to note that a work authored by this reviewer had acquired an entirely new place of publication. Nevertheless, this is a prodigious piece of scholarship and an important contribution to our understanding of the development of Russian primary education and its place in the history of Russia.

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ROBERTA THOMPSON MANNING. *The Crisis of the Old Order in Russia: Gentry and Government*. (Studies of the Russian Institute.) Princeton: Princeton University Press. 1982. Pp. xv, 555. \$47.50.

In Roberta Thompson Manning's view the "crisis of the old order" came about in the following way. Grain prices rose during the years after 1895, causing a fair number of gentry landholders to take up farming and become active in *zemstvo* assemblies. As a result the peasants, already desperately poor, became poorer. The gentry farmers became increasingly dissatisfied with the gentry bureaucrats who served the government, and a movement for constitutional reform commenced in the *zemstvo* assemblies. For a few months in mid-1905 these assemblies found themselves inadvertently endorsing political leaders who favored universal suffrage and even the forced expropriation of gentry farmland for distribution to peasants. Meanwhile, the peasants became violent. The gentry farmers in the *zemstvo* assemblies hastened to reject their liberal leaders, but not before the tsar granted Russia a semidemocratic constitutional government. In 1906, therefore, the *zemstvo* assemblies organized themselves to oppose the new government they had unwittingly helped to form. They succeeded. By June 1907 they had gained political mastery over the tsarist bureaucracy, and Prime Minister Stolypin, yielding to their pressure, restricted the electorate so as to give them predominance in the Duma. From then on, it was all downhill.

Manning has gone through an impressive mass of material relating to local *zemstvo* and gentry assemblies during the early 1900s. Much of what she says about them is interesting. It seems to me, however, that her work is altogether too narrowly focused to offer an explanation for the "crisis of the old order." Generally speaking, she studies only political activists organizing themselves in legislative assemblies to pursue ideological ends. They failed to achieve their ends; ergo, said most of them, Russian government and society were collapsing. And since Manning discusses only the activities and thought of these people, generally accepting their apocalyptic views as fact, she concludes that the tsarist government and its leading citizens were doing nothing but

collapsing after 1905 (pp. 346, 355–56, 370–71). Manning forgets that all participants in legislative assemblies are inclined to talk this way. She joins the enlightened but politically immature Russians of 1905 in endorsing George Washington's naive opinion that if political parties oppose each other and legislative chambers fail to reach a consensus the republic is doomed.

Indeed, Manning is determined not to see anything but doom. If some gentry farmers attended universities this was not a step forward for enlightenment but a direct cause for the fragmentation of the gentry into implacably hostile factions (pp. 49–58). When Prime Minister Stolypin compelled Tsar Nicholas II to pass legislation over the State Council's opposition in 1911, this was not a blow struck for parliamentary integrity but a demonstration of the Duma's helplessness (pp. 347–48). Surely it would sound strange for a historian of England to refer to Lloyd George's similar move against the House of Lords in 1911 as a sign of the House of Commons' paralysis, but this consideration does not trouble Manning. She eschews the frivolities of comparative history, preferring, as I am sure she would say, to stick to the evidence.

Manning's eagerness to impress her scenario on her readers leads her into the unfortunate practice of attaching general explanations in shotgun fashion to virtually every event she mentions. Many of them are valid, doubtless, but her solid analyses are weakened by being jumbled with less convincing remarks. Sometimes she seems to contradict herself. In one place she confidently opines that the government's harsh repressive measures against the peasants in 1905–06 broke peasant resistance (pp. 151–52), but a few pages later she asserts that the government's cruelty caused peasants to resist more fiercely (pp. 153–54). Soldiers returning from Manchuria are conceived in one place as a force to quell revolution (p. 139) and in another as a part of the revolution (p. 166). These are peripheral matters, but Manning's passion to fabricate general explanations sometimes leads her into very muddy water. In one grotesque instance she asserts that Stolypin's minister of agriculture, A. V. Krivoshein, was a tool of right-wing gentry groups. Her only basis is a few memoirs written by Krivoshein's political enemies (p. 361). She relies on newspapers and some remarks by the Soviet scholar Anfimov, an untiring defender of stale dogmas, to reach the conclusion that the funds Krivoshein's organization earmarked for agronomic aid to peasants actually went to gentry estates (p. 364). She should know better. Her own account shows that the "third element"—that is, the agronomists who were in charge of disbursing the aid—were inveterately hostile to the *zemstvo* assemblies and their gentry-farmer members.

One could forgive such flights of fancy if they opened up new perspectives, but this is not Manning's purpose. The viewpoint she commands her readers to adopt rests on well-known stereotypes. Imaginative writing does it more harm than good.

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A. IA. AVREKH. *Tsarizm i IV Duma: 1912–1914 gg.* [Tsarism and the Fourth Duma: 1912–14]. Moscow: Nauka. 1981. Pp. 291. 2 r. 60 k.

Recently Lawrence Stone has been good enough to let us know that historical narrative is back in style, but then again so are miniskirts and go-go boots. Unwittingly no doubt, A. Ia. Avrekh has offered us an example of such academic *rétro*. Yet this narrowly empirical and conceptually shallow account of Russian parliamentary politics before 1914 does not elicit much nostalgia for the old methods. A revolutionary situation emerged in Russia on the eve of World War I, but Avrekh fails to offer any deeper analysis of the causes and effects of increasing social instability. For someone who would profess to work within the Marxist tradition, Avrekh has produced a curiously hermetic account of high politics devoid of any serious attention to social roots.

To be fair, this monograph on the first two years of the Fourth Duma (1912–14) is, in some ways, superior to Avrekh's earlier writings on the Third Duma. He uses a wider range of sources (most importantly the police department's fund of intercepted letters), and he offers a far more detailed description of the competing political tendencies (especially those on the right). Yet Avrekh limits his attention to the two capitals and ignores conditions in the country at large. He uses the Moscow and Petersburg press not so much as a primary source of opinion and rumor as a secondary source for the support of his views. Too often he cites an unsubstantiated assertion in the Kadet organ, *Rech'*, as the unalloyed truth. Similarly, Avrekh exhibits a curious reliance on so idiosyncratic and isolated a commentator as *Novoe vremia*'s M. O. Menshikov.

Avrekh fails to analyze the results of the elections to the Fourth Duma. Only the results are given, and they are seen as the direct result of government desires and manipulation. Yet there is abundant archival material on this process, and the documents tell us much about the social character of the competing political parties. In ignoring these sources, Avrekh fails to come to grips with the central processes of prerevolutionary political development. He belittles and ignores the emergence of those political parties that sought to speak for a clearly defined class constituency. These groups

were the *Progressisty*, who were centered around the industrialists of Moscow, and the Nationalists, who drew their support from the Russian landlords of the southwest. Instead, Avrekh maintains his focus on the most visible parties of liberal and moderate opinion, the Kadets and the Octobrists, both of whom had their roots in the peculiar political environment of pre-1905 Russia. Avrekh describes in admirable detail the extensive disunity among the Kadets and Octobrists before 1914. Yet he fails to show that both parties' decline was the result of their failure to represent the interests of all but the narrowest and most amorphously defined constituencies. Many Kadets and Octobrists still maintained some sentiment for mutual activity as late as 1914. The *Progressisty* and Nationalists, in contrast, were far less amenable to compromising what were keenly felt interests. As a result, the instability among the propertied classes was more profound and serious than Avrekh describes.

To his credit, Avrekh does give us a good sense of the breadth of the *krizis verkhov* (crisis of the elites). He demonstrates the disarray of all parties, including those of the right. This fragmentation resulted in the Duma's failure to form a permanent working majority, and that paralysis came just at the moment that the tsar and the government were coming to abandon their remaining shreds of interest in or respect for the institutions of popular representation. The state's indifference alienated even the deputies of the extreme right. Avrekh clearly shows that the autocracy had little or no support among the propertied and educated social classes represented in the Duma. Instead of facing the rising tide of working-class opposition with unity, Russia's various elites were at each other's throats.

It must be said that Avrekh's story triumphs over his method. The deterioration of Russian society on the eve of the war was so thorough that even Avrekh's superficial approach cannot fail to lead us to inescapable conclusions. Yet his closet empiricism with tacked-on Marxist labels can hardly answer the more important questions of how and why this happened.

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DAN N. JACOBS. *Borodin: Stalin's Man in China*. Cambridge: Harvard University Press. 1981. Pp. viii, 369. \$25.00.

The fascinating and, until now, shadowy career of Mikhail Markovich Borodin (born Gruzenberg) has long awaited its biographer. The publication of Dan N. Jacobs's work fills that bibliographical void admirably, one could even say boldly.

Mikhail Borodin was one of the chief agents of the early Comintern and had his greatest moment as Soviet advisor to Sun Yat-sen and to the Kuomintang until the split with Chiang Kai-shek in 1927. His career left a clear mark on the history of the Communist International as well as on the political history of China, although it may be an exaggeration to refer to him as "the Bolshevik conqueror of half of China," as Jacobs does in his first line.

The early career of this merchant of revolution, before the China assignment, has long been masked by obscurity. In what is unquestionably the best part of the book, the first eight chapters, Jacobs carefully reconstructs that career. He follows Borodin from his birth in Vitebsk province in 1884 to his introduction to revolution in the Jewish Bund and his early adherence to Lenin. Borodin's life in the United States and his particular attachment to education are both well researched and well presented.

The future Comintern agent left the United States in July 1918 to return to Russia. There he was immediately drafted into the service of the revolution when Lenin asked him to return to America in order to carry his letter to the workers of America, a trip Borodin never completed. With the American trip aborted, Borodin worked in Scandinavia where, Jacobs argues, he demonstrated his organizational ability, a trait that got him appointed a Comintern agent soon after the institution's formation.

For the next several years Borodin was in the thick of revolutionary activity with foreign assignments in Western Europe, Mexico, and England, where he was held by the police for six months. Laying to rest an oft-repeated rumor, Jacobs concludes that Borodin was never in Turkey (p. 91); on another rumor-producing incident of Borodin's career Jacobs is less definite. On April 19, 1919, he left Moscow for the West supposedly carrying nearly half a million dollars worth of jewels. The jewels never made it to the U.S., a fact that would plague Borodin for years. Jacobs traces them as far as Haiti (p. 72) but avoids, probably wisely, a definite conclusion on the disposition of the jewels.

The author allots equal weight to Borodin's experience in China, chapters 9 through 16, as he did to the earlier career. It is here that Jacobs is at his boldest as a biographer. On very little cited documentary evidence, the author treats his reader to what "must" have been Borodin's, and sometimes even other historical actors', innermost thoughts. Interpreting between the lines of scarce sources, Jacobs displays an intimacy with his sources that will undoubtedly be questioned by some overly cautious historians. The boldness is appealing to this reviewer even given the fact that the author works entirely without Chinese-language sources.

The work is not without shortcomings. Some assertions of fact are questionable such as the flat

statement, without documentation, that fifty-one of fifty-six Kuomintang generals were large landowners during the start of the Northern Expedition (p. 216). There are a number of places where heavier citation would have been in order.

Further, the subtitle is misleading. It is true that Chang Kuo-t'ao, in the first volume of his translated memoirs, asserts that "in China, Borodin was the one who could best represent the views of Stalin" (p. 601). Jacobs, however, never clearly establishes any kind of particular relationship between Borodin and Stalin. The subtitle would suggest that such a relationship existed.

The shortcomings should not obscure the general high quality of the work. Jacobs has done an excellent detective job and has boldly stated his conclusions after sifting conflicting evidence. The bibliography is a fine introduction to the field of Comintern history. The work is a useful and significant contribution to the literature on the early history of the Comintern and on the Chinese revolution in the 1920s.

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NEAR EAST

JOHN OBERT VOLL. *Islam: Continuity and Change in the Modern World*. Boulder: Westview Press or Longman, Essex, England. 1982. Pp. xii, 397. Cloth \$30.00, paper \$13.95.

John Obert Voll adds to the current upsurge in the long-continuing discussion of Islam's ontological status, essence, and potentialities an analysis of the modern Islamic experience in its temporal and spatial totality, from the eighteenth century to 1980-81, from West Africa to the Philippines. The modern Islamic experience consists of "the interaction of the specific aims and goals of individuals and groups, which are affected by particular local conditions, with the factors of the dynamics of modern development and the continuity of the Islamic tradition" (p. 4).

Islam is "distinctively identifiable," but "not monolithic," rather it comprehends "common themes and diverse interpretations" (p. 21). There are four "styles of action" that "provide the network for the interaction of continuity and diversity within the Islamic experience" (p. 29). These are the adaptationist, the conservative, the fundamentalist, and the individualist. The styles are not separate movements. Instead, any individual or group may combine the styles with varying degrees of emphasis. Modern developments, from the eighteenth century on, comprise four phases, the latest of

which has just begun in the years around 1970. The phases are global in that the West has had a significant influence everywhere, but the Western impact has varied through time and space. This diversity of the Western impact reinforces other regional particularities, so that within each phase the general Islamic experience has varied from region to region. But throughout history, the Muslims have interacted in accordance with the four styles.

The eighteenth century defined the modern fundamentalist style; in the nineteenth century modernizing adaptation and fundamentalist reaction prevailed; in the twentieth century the dominant style has been "modernizing adaptation supported by a secularist individualism"; and just recently "the reassertion of the fundamentalist style has become an important force" (p. 355). It seems to this reader that the fundamentalist style is Voll's hero. He appears, not without ambiguity, to accept the common view that in the twentieth century Islam has come close to extinction in secularist modernism. This assessment has been favored by many advocates of Muslim causes, such as Arab nationalism, but the obverse is that Islam is intrinsically inadequate in the face of modernity, a charge that has been made by many. Escape from the predicament is afforded by the fundamentalist style, which promotes an indigenous sociomoral reconstruction that "is not simply a transplanted version of modern Western society" (p. 34), but "new and modern forms of the continuing Islamic vitality" (p. 2).

Voll applies his conceptual apparatus systematically and, usually, lucidly. In a number of cases, the treatment seems to this reader to be ambiguous and ambivalent, notably the assessment of the relative importance of Islam and secularism in the Arab experience. Surely, it cannot be otherwise. An effort like this must depend heavily on the existing secondary literature, and this is a very mixed lot. In synthesizing the literature Voll has labored heroically. He has also employed insight that comes from perceptive investigation of significant aspects of the Islamic past and present. The specialist will benefit from this attempt to isolate and define the Islamic element in the experience of the Muslim peoples. The general reader may have occasional difficulty in visualizing the concrete behind the general, but the diversity and complexity of the Islamic experience will be clear to all.

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NERMIN ABADAN-UNAT *et al.*, editors. *Women in Turkish Society*. (Social, Economic, and Political Studies of the Middle East.) Leiden: E. J. Brill. 1981. Pp. xii. 338.

In many respects this is an impressive volume. Written by fifteen women, all well trained, all teaching at Turkish universities, its very existence is testimony to the advances Turkish society has made in the past sixty years under the republic. Female emancipation was imposed from above, from Kemal Atatürk's own vision of what constituted a modern society. As Nermin Abadan-Unat notes in the introductory essay, there was hardly any mass movement for women's rights. But whatever the impetus, urban women in Turkey have compiled a surprisingly successful record.

Divided into four parts—"Population, Health, Nutrition," "Labour Force Participation, Education, Planning," "Continuity and Change in Sex Roles," and "Religion and Political Behaviour"—these essays are well introduced and reflect a degree of editorial unity that is not easily achieved in composite works. For brevity's sake I will discuss only the most suggestive chapters.

Şirin Tekeli's "Women in Turkish Politics" finds that for much of the twentieth century national political representation of women in congress and cabinet for the U.S. and Turkey was comparable. In Turkey, however, during the 1940s and 1950s when a multiparty system was introduced, female participation began to decline. According to Ayşe Öncü's "Turkish Women in the Professions: Why So Many?" during 1965-75 about 15 percent of physicians and 19 percent of lawyers were women. By contrast the U.S. figures in 1967 were only 6 percent and 3 percent respectively. She suggests that the rapid expansion of the professions opened the way for new elements, mostly upper-class women, who were less disruptive than lower-class men and that cheap female workers, and the extended family network provided house and child care. Deniz Kandiyoti, "Dimensions of Psycho-Social Change in Women: An Intergenerational Comparison," notes that elite Turkish women suffer less from "fear of success" than do American women as measured by Mattina Horner. Kandiyoti argues, "It would seem that the confusion between sexual and professional identity is most likely to occur . . . where women are in free competition with each other in the marriage market. Where family control over female sexuality is stricter and marriages arranged, the concern over femininity does not seem to appear as acute" (p. 246).

The significance of these findings is limited because the small urban elites of Ankara, Istanbul, and Izmir have little in common with the lower classes wherever they live. The shanty-town dwellers of Ankara can only dream of upper-class achievement; at best some may find work in factories and office buildings. Among small-town women even such dreams are alien, for their lives are limited to modest schooling, the home, and the occasional

company of other women, as Mübeccel Kiray sensitively portrays it. Moreover 70 percent of Turkish women live in the countryside, which legal reform has barely touched. It is upon the backs of peasant women that much of the economy of this agricultural country rests. And it is they who are oppressed in terms of nutrition, health, number of children, marital status, education, income, and harshness of labor. Unfortunately the book says little about them.

This neglect is not the only weakness. Despite the multidisciplinary training of the essayists (from public health to philosophy), there was no historian among them and a historical approach is absent from most of the essays. Thus Islam and the changing image of women in Turkish literature are both badly treated. All these failures ultimately reflect the dualism afflicting Turkey, which is divided between a Western-oriented elite and a traditionally minded peasant majority.

When compared to Lois Beck and Nikki Keddie, editors, *Women in the Muslim World* (1978), and Elizabeth Warnock Fernea and Basima Qattan Bezirgan, editors, *Middle Eastern Muslim Women Speak* (1977), this is a less useful contribution, though it does offer a corrective to the rosy view propagated by such earlier works as A. Afetinan, *The Emancipation of Turkish Women* (1962). The best single essay on the subject remains Fatma Mansur Coşar's in the Beck and Keddie anthology.

The work also suffers from the fact that it was written in Turkey, printed in Hungary, and published in Holland. In some essays pseudo-social science jargon (the new lingua franca) replaces independent research and thought. Typographical errors are abundant and sometimes misleading ("Paslett" for Peter Laslett [pp. 260 and 327]; Beck and Keddie's book is mistitled with names reversed and one misspelled [p. 313]).

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RICHARD G. HOVANNISIAN. *The Republic of Armenia. Volume 2, From Versailles to London, 1919-1920*. Berkeley and Los Angeles: University of California Press. 1982. Pp. xv, 603. \$35.00.

When Richard G. Hovannisian of UCLA published *Armenia on the Road to Independence, 1918* (1967) he envisaged another volume on independent Armenia, from May 28, 1918, to the end of independence late in 1920. But the study has expanded enormously. *The Republic of Armenia*, volume 1, *The First Year, 1918-1919* (1971) now is followed by the present, but only penultimate or possibly antepenultimate, volume. It covers eight months, from the declaration of May 28, 1919, that eastern and western Armenia constituted a single political unit, to the

opening of the Allies' London conference of February 1920, to work out the peace with the Ottoman empire.

In 1919 the Armenian republic, beset by all manner of problems, continued its precarious existence. Wardrop, British commissioner to Transcaucasia, found Erevan, the capital, looking "pitiful." Everything was in short supply, inflation raced on, and some six hundred thousand poor, over a third of them refugees from Ottoman lands, required a daily dole. Proposals to repatriate the refugees were never carried out, as no power would furnish the necessary military aid. Muslims inside the republic staged revolts, sometimes with Ottoman assistance. The government, dominated by the Dashnaks, struggled with these and other worries, including divergences among parties and splits within the Dashnaksutiun itself. Hovannisian devotes separate chapters to general conditions, repatriation, insurgencies, party politics, and government and administration. All are informative, the chapter on partisan politics particularly good. It gives a sympathetic picture of the Dashnaks, who leaned toward "territorial maximalism and a Western orientation" (p. 260), but the author never takes sides.

Even more than a domestic history, this volume is a history of Armenia and the outside world. Three of the fifteen chapters concern unfriendly relations with Georgia and Azerbaijan—territorial conflicts, minority questions, and a short war with Azerbaijan. There is also, later, an interesting section on the Armenian-Kurdish declaration of solidarity of November 20, 1919. Most of all, Hovannisian recounts what was done, or more often not done, for Armenia by the great powers. Much of the history of negotiations and arguments among Britain, France, Italy, and the United States concerning Armenia is familiar, but he has brought it together, in context, and supplied additional detail. The republic was finally recognized de facto by the three European Allies in January 1920; the United States followed only in April. That recognition was a somewhat anticlimactic substitute for the extensive political and military assistance that never came. One chapter examines the fleeting possibility of an Italian mandate in the Transcaucasus. Like that chimera, the other major events of 1919 were also negative: British troops withdrew, the United States declined to commit its own forces and decided against assuming a mandate either for Armenia or for any Ottoman territory. Armenian hopes had been pinned on Britain, then on America. The latter offered only "sympathy, flour, and fact-finding missions" (p. 315). France was eager to send troops, and did, but only so that it could control Cilicia, forgetting about Armenia. Hovannisian investigates the varying views within British and American officialdom and among lobbies, partisan groups, and mis-

sions, especially Harbord's, on what to do about Armenia. Finally, Britain and France had to elaborate the Ottoman treaty without the United States.

The work rests on superb research in Armenian, British, French, and United States archives and in published materials in Armenian, Russian, Turkish, and Western tongues. Armenian newspapers have contributed significantly. The bibliography (pp. 531–72) repeats much from volume 1, with additions. Hovannisian stays very close to his evidence, often summarizing documents. He furnishes perceptions of others, but sometimes fails to say what is the likely truth. For instance, he cites Azerbaijani newspaper reports that devastations by Armenians in the Zangezur district created forty thousand homeless Muslim refugees. We do not know whether this is true, half true, or untrue. Some readers will find the going hard, in places, owing to the summaries and the details. Others will be glad to have the realistic detail: cooperatives in the Armenian republic produced 40,000 kilograms of tomato paste in the 1919 season (p. 12); 903 of 1240 cases brought in Etchmiadzin's court had been settled by the end of 1919 (p. 292); the republic received 40,633.5 metric tons (475,043 barrels) of flour from the United States Grain Corporation, authorized in 1920 (p. 402). The footnotes make clear that the author has far more information available; the book is distilled, he says, from an 8,000-page typescript. In all this, one finds a careful objectivity, few errors of detail, and almost perfect proofreading—a line from page 138 is unfortunately transposed to page 139.

This is an impressive work. There is nothing so thorough or comprehensive in English, or indeed in any language, so far as I know, for any of Armenia's neighbors in these years, possibly excepting Russia—not for Georgia, Azerbaijan, Iran, Turkey, Syria, or Iraq. The total four or five volumes will be standard for many years to come.

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ERIC J. HOOGLUND. *Land and Revolution in Iran, 1960–1980*. (Modern Middle East Series, number 7.) Austin: University of Texas Press. 1982. Pp. xiii, 191. \$19.95.

Given the paucity of available studies in English on the Iranian agrarian reforms of 1962–63, Eric J. Hooglund's book is a welcome addition. It sets out to analyze the background of land reforms and their impact on the economic, political, and social structure of the affected rural societies. In part 1 the author draws heavily from published Western and Persian sources to give an exposé of the history of land tenure, rural socioeconomic conditions, the

origins of the idea of agrarian reform, and the implementation of an extensive program of land redistribution in the 1960s. In part 2 he relies more on his own field research to evaluate the overall significance and effects of the program.

Hooglund does not deny the "statistical success" of the program in terms of peasant land ownership. He argues, however, that "the actual positive benefits" of the program were "virtually nonexistent." He demonstrates that land reforms (1) did not alter the basic character of the agricultural system that allows the exploitation of the masses of peasants by a minority of owners, even though the composition of the latter group was modified; and (2) had an adverse impact on agriculture, causing a decline in production of food crops during the 1970s and mass migration from rural to urban centers. Hooglund asserts that these negative results were the direct consequences of the role played by the central government and the shah in the formulation, legislation, and application of the reforms. He views the shah's concern with land redistribution as primarily motivated by his need to control politically the villages and expand government role in rural affairs. In the last chapter he attempts to show how the shah achieved his goal and consolidated his authority through bureaucratic supervision, and manipulation of local elites and rival groups. In the conclusion the author discusses briefly rural participation in the revolution of 1978–79, which he claims to be the "first real test of rural attitude toward the role of the shah's government" (p. 138). He accounts for the peasants' failure to support the monarchy in terms of the apolitical attitude of most villagers, reflecting indifference, if not cynicism, to the fate of the shah's government. These villagers were then mobilized by local revolutionaries, mostly young men in their late teens or early twenties who daily commuted to the neighboring town for work, and thus were in contact with urban antishah activists.

Most of the book's analysis based on Hooglund's personal observation and interviews he conducted while in a village in central Iran in 1972 and in another in the south in 1978–79. As a case study, it is a valuable contribution, but it is neither a comprehensive study of land reforms in Iran, nor is it a detailed sociological analysis of rural politics. It does not offer the depth and breadth of Ann K. S. Lambton's description of the implementation of reforms in some three hundred villages. Nor does it provide the reader with a wealth of new empirical data as Iranian works on the subject (extensively cited by the author) do. Moreover, its evaluation of the program often tends to reflect Hooglund's political views, which allow no concession to the Pahlavi regime. For instance, Hooglund generally conceives the government developmental policies as

politically motivated, and readily dismisses its rural programs (education and health services, improvement of rural-urban means of transport and communications) as insufficient and inefficient—alienating forces intruding on rural communal life. Similarly he views village youth activism, and rural participation in the revolution in general, as an explosive by-product of dissatisfaction with the shah's government. He fails to note that the emergence of a politically aware rural group, like its urban counterpart, is a significant result of more widespread education reaching the lower levels of society, and of better means of communication that facilitated continual contact of the migrant peasant with his native village, and even allowed those who remained at home to commute to their urban place of work. Given the role Hooglund attributes to village participation in the overthrow of the monarchy (which is debatable since it was an extension of urban activism through the migrant or commuting village youth, rather than an independently organized movement from within), should one not see it as a measure of the success of developmental programs just as much as the urban middle class itself is an indicator of the success of modernizing policies of the Pahlavis?

A more serious shortcoming of the book is the absence of a theoretical framework for this case study. A comparative analysis of other developmental models in Asia, Africa, or Latin America could have enhanced our conceptual understanding of general obstacles to agrarian reforms both from within the rural communities and without. It would have helped the author provide a more adequate evaluation of the Iranian experiment.

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AFRICA

P. L. WICKINS. *An Economic History of Africa from the Earliest Times to Partition*. New York: Oxford University Press. 1981. Pp. 323. \$29.95.

During the past decade significant advance has been made in the writing and teaching about Africa's economic past. Despite the availability of some good regional and local histories, a textbook covering the whole continent has been sorely needed. Those teachers of African economic history who had expected P. L. Wickins's book to meet their needs, however, will be disappointed.

The book covers the economic history of the continent from the earliest times to the partition. The work is divided into five sections that cover agriculture, craft industry, trade, economic systems

of ancient and prepartition Africa, and the integration of Africa into the international economy. The study takes a partly topical and partly chronological approach. Within this framework, Wickens provides a detailed account that draws much of the recent research on the subject. In spite of the considerable effort that must have gone into the work, the result is not a successful synthesis.

Although the book is generally accurate and the approach is balanced and dispassionate, it suffers from a number of serious defects. Major problems are the lack of informed methodological considerations and the absence of a theme or themes that hold the vast materials presented in the book together. The book is also chronologically confusing. There are many sections of the book that jump back and forth between the millennia B.C. and the nineteenth century A.D. The movement from region to region and the introduction of places in the text without adequate maps are bewildering. Statistical data are included in the text without adequate discussion or explanation. A comprehensive book covering precolonial economic history of Africa has yet to be written.

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RUTH BERINS COLLIER. *Regimes in Tropical Africa: Changing Forms of Supremacy, 1945–1975*. Berkeley and Los Angeles: University of California Press. 1982. Pp. x, 221. \$26.00.

This book deals with the way in which the introduction of electoral politics in Africa helped or hindered the emergence of a cohesive political elite. The study is based on a comparative examination of twenty-six African countries, from Mauritania to Malawi. The territories in question effectively correspond to that part of the continent where independence was achieved through what is euphemistically known as a "negotiated process"—that is, a transition from colonialism to neocolonialism guided, more or less gently and effectively, by the imperial powers. The period covered is that of decolonization and "immediate post-independence": in practice a thirty-year stretch (1945–75) appropriately bisected by the year 1960. The unit of analysis is that of the national-political regime (defined as "the structure of formal, legitimate power"), and the key variable is electoral participation, which, although it represents only one form of political participation, has the merit of being readily quantifiable.

Ruth Berins Collier argues convincingly that scholarly disillusionment with the study of regimes in Africa may have led to a neglect of national or domestic politics. Yet, it is precisely because "a given

type of economic system and the pattern of class relations that accompanies it can coexist with, and be maintained by any of a variety of types of regime" (p. 7) that a study of regimes is important in answering the question of how a given type of social system is supported. If they cannot be accounted for by fundamental differences in the economic base, how then are the differences in the legitimating structure of the state to be explained? Collier's thesis is that part of the explanation has to do with the degree of cohesion of the political class resulting from the specific ways in which institutions were transferred from the European metropolises to Africa and with the different experiences that each territory or group of territories had with electoral politics during the decolonization period. Along that line, she argues that such factors as mass mobilization, electoral participation, types of party system, and types of colonial rule—all of which have been regarded in recent studies as somewhat outmoded or irrelevant—possess explanatory power.

Collier observes that elections and electoral participation were introduced in Africa "at a faster rate and at a much lower level of socio-economic development than in Western Europe" (though the gap between Europe and Africa is not nearly as wide as is commonly assumed) but that the rate and sequence of these transformations varied substantially from one African country to another.

Based on a careful analysis of the data, Collier finds that "the level of electoral participation attained by independence was not stimulated primarily by the level of mass social mobilization" (p. 92); nor, she claims, was the relatively high level of participation as disruptive of political cohesion and elite interests as has often been asserted: "rather, level of participation tended to be an outcome rather than a cause of elite political cohesion." In French Africa, high levels of voting corresponded to the plebiscitary mobilization of support (and to the bandwagon effect) elicited by a highly dominant party. Among British colonies, however, this was generally due to the mobilization of the vote by a fragmented, often locally based elite in a context of greater party competition. Such factors as educational policies, the legacy of direct versus indirect rule, electoral procedures, and (in the case of East and South Central Africa) the existence of white settler minorities help account for such differences.

In the postindependence period, there was a rapid dismantling of the multiparty competitive regimes, which, although they had been tactically useful during the decolonization exercise, were "inappropriate mechanisms for the emerging African elites to consolidate political rule" (p. 114). Yet, patterns of postindependence regime change were directly related to a territory's experiences during the decolonization period. Those countries where

the introduction of Western electoral procedures resulted in the monopoly of power by a dominant party under conditions of free competition were those where the transition to one-party regimes (even when perpetuated by repressive means) met with the least amount of challenge. Those where the same formal outcome was accomplished through a merger between leading parties have been only slightly less successful. But countries where a one-party system was achieved through various forms of manipulation or coercion (single-list elections, banning of opposition parties, and so on), and even more obviously those where political fractionalization made it impossible to form one-party regimes experienced the highest levels of regime instability (usually signaled by military intervention).

The authoritarian regimes that proliferated throughout Africa after independence had the effect of limiting pluralism and of eliminating opposition in their attempt to consolidate the supremacy of the ruling elites. They effectively disenfranchised the masses and thus restricted the access of potential counterelites to the key resource of popular support. Yet civilian regimes (and even military regimes in some cases) generally opted for retaining some form of election. In former British territories, this has usually taken the form of one-party competitive or semicompetitive elections (notably through the system of multiple candidacies, which has been used regularly in Tanzania, Kenya, or Zambia), while the characteristic mode in former French Africa is that of the noncompetitive, one-party election, which can more appropriately be defined as a plebiscite. Both types perform a legitimating role—the latter through a ritual demonstration of mass support, and the former through what Collier characterizes as "rituals of choice" while recognizing that the degree of choice can, in fact, be quite significant. Competitive one-party elections also represent an effective vehicle for upward communication from the grassroots to the center, whereas elite-mass communication in plebiscitary systems runs almost entirely from the top down.

Using sets of variables referring to preindependence electoral patterns (levels of party dominance and of electoral participation) and to the transformation of party systems after independence, Collier identifies five model patterns of political change. With some notable exceptions, such as Ghana, Chad, and Uganda (or Rwanda, which does not fit into any of the five categories), these patterns correspond rather closely to regional groupings: the Francophone states of the upper Guinea coast, those of the lower Guinea coast (down to the Congo), the Francophone landlocked states, the former British "settler territories" of East and Central Africa, and a somewhat more amorphous or residual category covering most of former British

West Africa together with Zaire, Burundi, and Uganda.

This summary of the major findings of Collier's book cannot do full justice to its sophistication and honesty. Collier acknowledges the fragility and the approximative nature of some of her correlations, and she extensively resorts to traditional descriptive or analytical studies to support or account for both the patterns of similarity and the aberrant cases that emerge from her treatment of electoral data. The nature of that data base offers little opportunity for effective critique and does not allow for the consideration of such factors as the stakes involved in different elections or the various forms of manipulation that may have taken place (notably in French Africa) during the ostensibly "free" elections occurring before independence. Differences between some of the regime types may be somewhat overstated as are the extent and significance of the preindependence electoral experience in French Africa. These are, however, minor shortcomings that do not detract from the merits of this excellent work. Readers should keep in mind that Collier's study concerns only one form of political participation and avoid (as she does) drawing unwarranted extrapolations from her findings.

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HARRY A. GAILEY. *Lugard and the Abeokuta Uprising: The Demise of Egbas Independence*. London: Frank Cass; distributed by Biblio Distribution Center, Totowa, N.J. 1982. Pp. x, 138. \$30.00.

Harry A. Gailey is known to African specialists for his *History of Africa . . . to 1800* and for his works on the Aba riots and Gambia. His political biography of Governor Cameron focused on his interest in British colonial administration in West Africa; the current study retains this focus although with a different protagonist. In this work Gailey takes on no less a personage than Lord Lugard, whose name looms large in the annals of colonial history but whose myth has been eroded by the recent generation of African historiography. Gailey discusses his changed attitude to Lugard, coming to the conclusion that—as far as his stewardship of colonially federated Nigeria is concerned—Lugard was a mediocre administrator whose failure to understand the political and social structures of the Egbas resulted in the uprising of 1918 and its heavy toll of lives. This scenario, with a different roster of actors playing at different locations, can be applied to numerous other uprisings in colonial Africa.

Gailey's thesis rests on a complex set of circumstances that commenced with the enlargement of Lagos (British) influence at Abeokuta after the

treaty of 1893 had seemingly recognized the independence of that city-state. Compounding this was the mistaken British view of the *Alake* as Egbas monarch. Then, with the appearance of Lugard, came the decision that administration in southern Nigeria was to be modeled on the system employed in the Islamic north, but a continuing difficulty in the application of these techniques was the independent Egbas state on the Lagos doorstep. Lugard then searched for reasons to abrogate the agreements. Finding them, he proceeded to rationalize Egbas administration to the point that, with the introduction of direct taxation in 1917, the Egbas exploded. Gailey argues the case closely, based on his ready familiarity with the appropriate Colonial Office records.

It is a persuasive argument from the perspective of colonial administration. But to the eye of this reviewer it is not wholly satisfying, resting as it does in a mechanistic way on British action and Egbas reaction. While Gailey offers a few glimpses of Egbas society, they are insufficiently developed and do not take into account the dynamics of that society. The Egbas had been in a state of flux since the 1840s, with continuing wars, new modes of production, and increasing integration into the Eurocentered economic system. New classes and interests had been emerging, witnessed not only by the rise of a military caste but also by the appearance of the Abeokuta Commercial Association, the Egbas United Board of Management, and the Egbas National Council. Indeed, much of modern Egbas history can be seen in terms of the continuing attempts of these new groups to secure their interests vis-à-vis traditional political and economic elements. This side of the 1918 uprising is left unstated.

The book is well written and produced, although the single map is deficient in identifying Egbas place-names, let alone the surrounding Yoruba and Fon country.

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JOSEPH E. HOLLOWAY. *Liberian Diplomacy in Africa: A Study of Inter-African Relations*. Washington: University Press of America. 1981. Pp. xxiv, 230. Cloth \$21.25, paper \$11.00.

By postulating an overwhelming impact of Liberia's domestic relations on diplomatic relations with African nations, Joseph E. Holloway's study seems designed to accentuate three essential points: (1) the colonial character of relations between repatriate and indigenous Liberians; (2) the inherited Southern United States antebellum master-slave mentality of William V. S. Tubman's leadership; and (3) the

colonial or antebellum mentality of Tubman's approach in neutralizing the Kwame Nkrumah-promoted African Union Government.

Once the American Colonization Society gave power to its subjects (repatriates), the latter proceeded systematically, we are told, to re-create in Africa the master-servant relationship experienced in America, themselves becoming the masters and the indigenes the servants. With the advent of African independence the Liberian leadership simply transferred to its relationships with other African leaders the attitudes it had cultivated at home with its indigenous brethren. Political progressivism had to be curbed abroad as it had been at home in the interest of maintaining the status quo within and without.

Against this perspective the drive for organizing African unity is portrayed as a struggle between Nkrumah's progressivism and Tubman's conservatism. But Tubman's conservatism is not to be understood in the conventional sense as, for example, the Houphouët-Boigny-Nkrumah wager of 1957. Rather, Holloway would have us view it as the extension abroad (in Africa) of the domestic policy of expanding repatriate privilege to the detriment of indigenous rights. Tubman's conservatism was then only a means of neutralizing what was perceived as a dangerous external environment that could contaminate an effectively controlled domestic situation.

Elliot Berg aptly observed that "Liberia seems to inspire in those who observe and write about it a peculiar polarization of attitude" (*Liberian Studies Journal*, 2 [1970]: 175). One is either portraying in superlatives the virtues (or vices) of a repatriate community or those of an ethnic community. Holloway seems to follow this tradition.

While he does present a generally enlightening account of Liberia's African relations, it appears that the problem of attitudinal polarization referred to above led to assertions of definitive conclusions when often more probing and comparative analysis was required. In suggesting, for example, that Tubman's articulation of the fears of the rest of Africa doomed the United States of Africa to stalemate, is one not attributing too much to the man and his small, virtually powerless state? It must be remembered that there were in the 1950s six other states besides Liberia and Ghana. How did such conservative states as Ethiopia, Morocco, or Libya react to Nkrumah? It would be helpful to look therefore (comparatively or cumulatively) at other forces at work on the continent, both those that represented European colonialism's legacy (elite-mass cleavages) and the tremendous impact of the Cold War.

A final important contention made by Holloway is that Liberia's policy cannot be understood exclusively in an African framework because of the predom-

inance of the antebellum influence. It becomes "virtually meaningless to talk about Liberian foreign policy and domestic policies as if they were a purely African phenomenon" (p. 181). But this reality of external influence in the polity seems to exist in virtually every African country, for Europe's legacy is everywhere to be found.

By reviewing the roots of the unequal and discriminatory domestic relations in Liberia and suggesting that they affected foreign policy, Holloway contributes to our understanding of the motives and processes of Liberian foreign policy. When he goes on to suggest, however, that Liberia's domestic relations projected abroad decisively shaped the course of continental relations as institutionalized in the Organization of African Unity, he engages in that attitudinal polarization that academic studies on Liberia should now be moving away from with the effective termination of repatriate oligarchy.

D. ELWOOD DUNN

University of the South

HAGGAI ERLICH. *Ethiopia and Eritrea during the Scramble for Africa: A Political Biography of Rās Alulā, 1875–1897*. (Ethiopian Series, number 11.) East Lansing: African Studies Center, Michigan State University or Shiloah Center for Middle Eastern and African Studies, Tel-Aviv University, Tel-Aviv. 1982. Pp. x, 221. \$11.75.

Ras Alula was one of the most notable provincial rulers of nineteenth-century Ethiopia. The son of a peasant, he became a follower of Emperor Yohannes IV, who, recognizing his ability, integrity, and loyalty, entrusted him with the governorship of Marab-mellash, later to become the core of the Italian colony of Eritrea. As ruler of this important region Alula was responsible, in the mid-1880s, for the recapture from Egypt of the Keren area. Subsequently, on marching toward the port of Massawa, he declared that he would not return until his horse had "drunk from the Red Sea." He later established his camp at Asmara, where, by encouraging Muslim merchants to settle, he developed a major commercial center. Building up an army of 15,000 riflemen, he began to pay them salaries, thereby terminating the age-old practice whereby warriors lived by plundering the peasantry. He also attempted as far as possible to mobilize his men only when this would not interfere with their work on the land. Such policies earned him popular esteem as evinced by a saying still current, which brackets him with a famous medieval emperor, "justice like that of Lalibala; legislation like that of Alula."

Ras Alula's reputation rests, however, as his first biographer, the Israeli Haggai Erlich, recognizes, on being one of the greatest of Ethiopia's patriotic

leaders. As governor of the country's northernmost province he was directly affected by the European scramble for Africa. Confronted with Italian incursions from Massawa in 1887 he attacked, and virtually annihilated, an Italian battalion at Dogali. The "massacre," as the Italians chose to term it, caused a government crisis in Rome and made Alula virtually a devil in Italian eyes. The present reviewer was told by Dr. Nystrom, a Swedish missionary brought up in Italian Eritrea, that as a child if he misbehaved he would be threatened that the Ras would punish him. The governor of Marab-mallash was no less renowned in the Sudan, where a Mahdist historian referred to him as "a bone in the throats of the British, Italian and Turkish empires."

Alula played an equally decisive role in 1896. Having learned, through his spies, that the Italians were planning an assault by stealth on Emperor Menilek's army, then encamped near Adwa, he passed this information to the Ethiopian ruler and thereby contributed substantially to his country's victory. This triumph much enhanced Ethiopian prestige and caused the Italian author Bizzoni to exclaim in a famous phrase, "Who has ever heard of victories over the Abyssinians?"

Erlich's book, based on a London University PhD thesis, is a valuable work of scholarship. Its author has long been engaged in a by no means uncritical study of Alula and deserves much credit for unearthing a contemporary Ge'ez manuscript biography of the Ras—which he published in the *Bulletin of the School of Oriental and African Studies* in 1976. The present text, perhaps on account of limitations of space, nevertheless fails at times to set Alula in his full cultural context so that the reader is left unaware how far the hero reflects the traditions of the Ethiopian ruling class and how far he deviates from them. It would also have been interesting if Erlich had included an assessment of the accuracy of the many Italian reports on which much of the work is based.

The book, as its title implies, is in some ways far more, but in others somewhat less, than a biography of Alula. Erlich surveys skillfully, and in considerable detail, two of the most eventful decades of Ethiopia's diplomatic history, 1875–97. This leads him to discuss willy-nilly the policies of two successive Ethiopian monarchs, Yohannes and Menilek, two invading powers, Egypt and Italy, and three other countries interested in the area, Britain, France, and Russia. Though Erlich focuses his attention on Alula throughout this formative period and has unearthed a considerable mass of detail on the Ras's life for which scholars will be grateful, the inner personality, and springs of action, of the victor of Dogali remain something of an enigma—a fertile field perhaps for the psychological biographer, as well as a challenge—and a source of invaluable

documentation—for Ethiopia's own historians and men and women of letters.

It is to be regretted that the publishers have inadvertently omitted the three maps, an index to which is provided, also that the principal index, though useful, is far from complete.

RICHARD PANKHURST
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LEE V. CASSANELLI. *The Shaping of Somali Society: Reconstructing the History of a Pastoral People, 1600–1900*. (Ethnohistory.) Philadelphia: University of Pennsylvania Press. 1982. Pp. xvi, 311. \$25.00.

Despite its growing international significance in recent years, the Horn of Africa has received scant attention from historians. This has been particularly true for Somalia, whose precolonial history has remained one of the least known in sub-Saharan Africa. Part of this inattention has been due to the difficulty of reconstructing the early history of a nomadic, decentralized society living in a harsh ecological environment. Lee V. Cassanelli's investigation of the development of Somali society can be considered a pioneering effort, therefore, in at least two distinct ways. As the first major study of precolonial Somalia, it represents a welcome addition to our previously meager substantive knowledge. And as one of the first attempts to reconstruct the early history of a nomadic pastoral community, it represents an interesting exercise in historical methodology.

A wealth of substantive information, enlivened with strikingly appropriate proverbs, is contained in the fourth, fifth, and sixth chapters. These chapters respectively deal with the Islamization of Somalia, the development of nineteenth-century southern Somali economic activity, and the resistance of the Somalis of the Benaadir region to the coming of colonial rule. In each of these chapters, Cassanelli employs an effective combination of documentary and oral sources, particularly in those on Islam and on resistance. All three chapters have historiographical implications that go beyond an investigation of Somalia itself and make important contributions to basic themes in precolonial African history.

Criticisms of these chapters are minor. It is regrettable, for instance, that the author did not include more specific information on the Ethiopian imperial thrust in chapter 6. Likewise, the inclusion of comparative information on other nineteenth-century East African commercial systems would have sharpened his analysis of Somali long-distance trade in chapter 5. But such criticisms hardly dilute the major substantive contribution he makes.

More serious reservations can be made regarding his treatment of methodological concerns, however.

Despite Cassanelli's assertion that "the book can be considered as much a study in the methodology of nomadic ethnohistory—and oral history—as it is an attempt to add to our substantive knowledge" (p. ix), surprisingly little direct reference to oral materials is made in the main body of the text, with the notable exceptions of the fourth and sixth chapters. Indeed, his main treatment of the nature and analysis of oral materials rather curiously is relegated to an appendix. An analysis more closely integrated into the development of his main historical themes surely would have been preferable to this separate and rather abstract treatment.

It is also unfortunate that his oral materials did not figure more prominently in the introduction and the second chapter, where methodology is the chief focus and where the author describes and defends his "regional approach" to Somali history. His main concern here is to find an approach that will allow him to detect broad patterns of historical change from fragmentary data: the fundamental problem confronting any historian attempting a reconstruction based largely on oral tradition.

Many other oral historians have approached this problem through a process involving the thorough collection and analysis of traditions, particularly those of the "local" variety, while at the same time striving to assess them within their own particular environmental and cultural contexts. For an excellent treatment of such processes see Joseph C. Miller, editor, *The African Past Speaks* (1980); and, among others, Thomas Spear, *The Kaya Complex* (1978); Godfrey Muriuki, *A History of the Kikuyu, 1500–1900* (1975); and John Lamphear, *The Traditional History of the Jie of Uganda* (1976). Noting the undeniable frustration inherent in the interpretation of such "local" traditions, Cassanelli adopts a uniquely different approach. First he constructs a model of strategies employed by Somali nomads to deal with droughts in the twentieth century. Then, the regional framework that emerges is projected back into the precolonial setting as a unit of analysis from which, the author argues, can be derived insights into processes of historical change only vaguely perceived from the oral and written materials at his disposal.

Apart from the obvious danger of extrapolating evidence from a modern period to reach conclusions about an earlier time, the approach is questionable in other respects as well. The focus on droughts, for instance, seems largely to ignore other cataclysmic circumstances, such as raiding pressures or epizootics, that clearly would have elicited very different adaptive strategies, especially in a precolonial setting. Moreover, the very concept of pastoral migration implicit in the model appears to be one of dramatic movements of human populations rather than a process of cultural transmission or, at most,

of subtle territorial drift, as is so often the case with African pastoral communities. Again, a careful analysis of the Somali oral traditions themselves, integrated into this interesting regional approach, might well have avoided such problems.

Nevertheless, Cassanelli's singular approach appears to have worked remarkably well in some important ways. The model does, for example, allow the author to gauge the maximum territorial and social extensions of the *reers*, the herding units that are the fiber of Somali society. This, in turn, gives him a clearer insight into the ecological and social bases of regional sultanates that appeared in Somali society from time to time. It even serves as the basis of the interesting observations he makes concerning contemporary Somali history in the concluding "Afterword." Again, though, more frequent reference to the oral materials themselves would have allowed for an even greater florescence.

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JEFFREY A. FADIMAN. *An Oral History of Tribal Warfare: The Meru of Mt. Kenya*. Athens: Ohio University Press. 1982. Pp. 185. Cloth \$20.95, paper \$7.95.

Few subjects are more misunderstood than precolonial African warfare. This study focuses on the last decade (1896–1906) of decentralized warfare among the Meru, who inhabit the northeast slopes of Mt. Kenya. While paying tribute to Meru's former warriors, Jeffrey A. Fadiman reconstructs the design and purposes of Meru's system of warriorhood that provoked conflict while limiting its intensity. In the context of explaining the meaning of becoming a warrior and the role of warriorhood in Meru life, Fadiman's central chapters discuss the *process* of becoming a warrior: learning warriorhood, the experience of the novice warrior, waging war, and the transition from warrior to elder (chaps. 3–6).

We do not learn until page 42 the purpose for war in Meru. They fought to acquire cattle (for bridewealth, for rituals that reinforce relationships, and for sacrifices to remove impurities) and in so doing, acquire praisenames and status, bringing honor to the family. Fadiman's greatest contribution, in my opinion, is to demonstrate how such emphasis on warriorhood with insistence on cattle raiding did not disrupt Meru life but, instead, was a part of a larger system that led to harmony.

Warfare was highly controlled and never waged indiscriminately. Elders controlled the training, procedure, and spoils of warfare. In raiding for cattle, according to Fadiman, warriors provoked conflict in their culturally sanctioned desire for self-develop-

ment. They learned to limit and regulate conflict as they gained self-control and subordination to the group. They actively resolved conflict and created larger harmony between conflicting units as they willingly allowed subordination to age and tradition.

Warriors had a perpetual dependence on their elders economically, because their fathers (or same age-set kin) received all the cattle from raids. Only fathers could provide warriors with the means for bridewealth or to relieve ritual impurities. Warriors' acceptance of elders' approval and control as *protection*, not punishment, was drawn directly from Meru belief that the "ultimate source of [elder] authority lay in neither their secular abilities nor their social position, but in the very fact of their advancing years" (p. 137). This belief, therefore, fostered integration; submission to the restrictions of the Meru system formed the most certain path to power. The inevitability of political alternation gave every member of the system the assurance that his group would eventually achieve all forms of social status and control. Rewards were reserved for elders and all elders attained them.

This analysis seems a little too certain, too right, and one is unclear which are Fadiman's certainties or those of Meru elders. Still, the concentration on a single, if final, decade allows thoroughness, and this study is rare in its comprehensive linkage of warriors and councils of elders and of conflict and integration. It suggests further study both to examine Meru's neighbors and to follow the Meru through the colonial period to see how such a regularized system survived when, over time, neither age nor tradition could be maintained as the ultimate source of authority.

This book has an excellent appendix on methodology but would have been greatly aided by a glossary and an index. We can be grateful that Fadiman collected and analyzed these Meru traditions for now is truly the "final decade" to capture the oral history before the elders, whose grandsons would rather learn at school, die with these traditions untold.

CYNTHIA BRANTLEY
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ANDREW COULSON. *Tanzania: A Political Economy*. New York: Clarendon Press of Oxford University Press. 1982. Pp. xiv, 394. Cloth \$34.95, paper \$15.95.

This book falls somewhat between stools. Given the subtitle and the author's professional qualifications (he is a development economist), one would expect him to offer yet another in the growing body of

essays on recent efforts to implement "African socialism" in postcolonial Tanzania. Andrew Coulson announces in his introduction, however, that his major goal is "the study of economic history and colonial policy" in order to "shed light on contemporary Tanzanian decision-making" (p. v). About one-third of the book is historical in this sense, but it is not the best part. Coulson openly acknowledges his debts to others; but his knowledge of the literature here is not as thorough as for the later sections, and his analysis adds little to the work of such earlier authors as John Iliffe, Goren Hyden, and E. A. Brett, who have already provided an overview of the development issues in the period preceding independence.

Coulson comes more into his own after he reaches the post-World War II phase of colonialism when major undertakings, such as the notorious "Groundnut Scheme" of the late 1940s, foreshadowed the ambitious and general failures of the later nationalist government under Julius Nyerere. For the period since independence, Coulson possesses a really authoritative command of the secondary and primary materials, and has also benefited from his own experience as an agricultural advisor and university lecturer in Tanzania. Indeed, his book is not only a valuable treatise on the political economy of this era but also direct evidence of its rare combination of radical efforts at social transformation with the cooperation and critical witness of a wide range of expatriate and indigenous intellectuals.

Like many other critics of both the left and right, Coulson blames the shortcomings and abuses in Tanzania's various programs for semicollective rural development and urban industrialism on the role of state and party political authorities, whose interpretation of socialism seemed to involve a compulsion to keep all power within their own hands. Thus, a certain note of regret enters into the account of "kulaks," that is, local capitalistic farmers who, at obvious expense to rural egalitarianism, were able to modernize agricultural production and increase output during the 1950s and 1960s. There is even more overt antiregime sympathy expressed in the lengthy account of the Ruvuma Development Association, a rural project that fully met the criteria of both equality and efficiency but was destroyed because of its independence from central authorities.

While offering no defense of state policies in agricultural development, Coulson feels that the failures in industrialization (which he himself has chronicled in detail elsewhere) represent poor execution and foreign sabotage of an essentially correct policy. In places he seems a bit naive in insisting that a poor, relatively small country like Tanzania should be able to develop an intermediate goods sector (particularly steel production) as a basis for autono-

mous industrialization. He does, however, point out some of the difficulties in such an undertaking.

Even on those issues that Coulson knows best, his book is neither outstandingly original in its data and analysis nor elegant in its narrative and argument. Nonetheless, in contrast to the other authors on whom he depends and with whom he argues (notably Goren Hyden on peasants and rural socialism and Justin Rweyamamu on industrialization), Coulson maintains a valuable balance between non-sectarian sympathy for the socialist aims of the Tanzanian regime and criticism of its very obvious failures. His historical perspective seems to have been of some value here, even if it is not developed very explicitly. This work is not destined to become a classic, but it is the best account available of a very important episode in recent African history.

RALPH A. AUSTEN
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PETER RICHARDSON. *Chinese Mine Labour in the Transvaal*. London: Macmillan; distributed by Humanities Press, Atlantic Highlands, N.J. 1982. Pp. xiii, 287. \$31.50.

Misled by its title, put off by its cumbersome prose, or puzzled by its varied perspectives, many historians may ignore or skim this book. That would be a pity. The fate in the Transvaal of 63,000 Chinese laborers imported between 1904 and 1907 to work in the Rand gold mines takes up less than twenty pages. It is their function in a much broader context that preoccupies Peter Richardson. He treats them as "commoditised and internationally circulated . . . labour power" (pp. 2-3). In a world economy dominated by expansionary capitalism, able to harness technological innovation and control both core and peripheral state power, these workers are denied virtually all the value their labor produces. This claim does not rest on heavy-handed use of neo-Marxist methods. Orthodox economic analysis intrudes, and techniques used in conventional policy-making studies in imperial history are employed.

Much of the book is taken up with recruitment. The laborers came predominantly from north China rather than the southern districts from which most overseas Chinese workers emigrated. Their status, as well as their origins, and the pressures and inducements that sent them to coastal ports are finely examined with the aid of laboriously wrought tables and are illuminated by several maps. The role of gold in international economic activity and the southern African labor market are among other subthemes handled.

In examining the industry's strategies Richardson might have done more with differences in method

and performance of the several groups that controlled the mines. Table A.20, for example, lists mines by group but does not include capital issues, production figures, or dividend yields. These varied considerably. One wonders if some houses were more crudely exploitative than others. The two most serious Chinese protests against their lot occurred in mines controlled by two of the worst-run groups.

The subtheme that gets the most sustained attention is the interaction of both business and government at the level of individuals, institutions, and structure. Chinese and British officialdoms are probed to see to what extent they were willing tools of capitalist agencies: the findings are not straightforward. The capitalist juggernaut, while seldom thwarted, was often tamed or deflected. The regulations British bureaucrats imposed to ensure the health of the Chinese laborers had clout. Along with the capitalists' own efforts to protect their investments and the availability of fast steamships, the enforcement of these rules ensured much better care than previously found on ships used in the labor trade.

Richardson's approach to capital-state relations is best revealed in his treatment of the termination of the experiment. It wound up in 1910. Why, one might ask, did the British and Transvaal governments force the mining industry to give up Chinese labor? Or, as Richardson phrases the question: "why did the industry acquiesce in the decision to repatriate the labour force . . . ?" (p. 182). His answer to his question is nuanced, substantiated, and plausible. But his evidence can also be used to answer the other question, an indication that this book should be savored by a wide cross section of historians whatever their ideological and methodological preferences.

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ASIA AND THE EAST

ADAM YUEN-CHUNG LUI. *The Hanlin Academy: Training Ground for the Ambitious, 1644-1850*. Hamden, Conn.: Shoe String Press. 1981. Pp. xv, 284. \$25.00.

Adam Yuen-Chung Lui returns in this monograph to a subject in Chinese history that was popular in the 1950s and 1960s. *The Hanlin Academy* examines Chinese patterns of official appointment and promotion by focusing on this central government agency during the first century and a half of Ch'ing rule (despite the title, most the analysis covers only the period from 1644 to 1795).

The Academy was a prestigious unit, a first appointment for those who did best in the palace examinations. Its members performed as imperial lecturers and advisers, tutors for the princes, compilers of officially sponsored scholarly projects, and examiners who supervised the civil-service examinations. Lui studies the origins of the Hanlin, its emergence to prominence in the Ming dynasty, and its role in early Ch'ing officialdom. The heart of the book consists of a statistical analysis of Hanlin career patterns, based on a sample of the over two thousand six hundred persons who passed through it between 1644 and 1795. In most respects, the scholars appointed to the Hanlin Academy resembled the larger group of *chin-shih* (winners of the highest examination degree) in terms of their native place, age, and family background. Here and at other points Lui's work is profitably compared with P. T. Ho's study of degree winners, *The Ladder of Success in Imperial China: Aspects of Social Mobility, 1368-1911* (1962), which is still the magisterial work on this subject. Lui goes on to analyze patterns of promotion among Hanlin officials. Groomed for central-government postings, Hanlin scholars were indeed promoted more rapidly and eventually reached higher ranks than ordinary *chin-shih*. Seniority was a major but by no means the only factor in promotion to the highest ranks; as anticipated, the top three scholars in the palace examinations generally did better in their official careers than the rest. In contrast to native place, which did play an important role, family background was of marginal importance in promotion to senior ranks. Early Ch'ing personnel policies favoring Bannermen, members of the Manchu Eight Banners, and northern Chinese are reflected in statistics on Hanlin appointments and promotions. Appendixes providing information on probationary periods for non-Hanlin officials, translations of titles of early Ch'ing offices, and brief biographies of 138 Hanlin scholars conclude this work.

Those who wish to scrutinize the longest continuous bureaucracy in history will find many details in this book that will interest them. Although Lui's explanation of how he selected the sample that formed the basis for most of the statistical analysis is not precise enough to enable readers to judge just how representative it really is, the data presented in the many tables should be useful for those who are interested in comparative analysis. When we seek to understand the animating forces in Ch'ing bureaucracy, however, we must turn to topics that are not really covered in this book: regional-mobility strategies, the influence of specific rulers on political events, and concepts of statecraft debated during the early Ch'ing.

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JOHN W. WITEK. *Controversial Ideas in China and in Europe: A Biography of Jean-François Foucquet, S.J., 1665-1741*. (Bibliotheca Instituti Historici S. I., number 43.) Rome: The Institute. 1982. Pp. xv, 494.

Jean-François Foucquet was one of the French Jesuits sent to China by Louis XIV in the late seventeenth century. Between 1698 and 1720 he worked both in the provinces and at K'ang-hsi's court, always amid some tension and controversy with his superiors and confreres. He was recalled to Europe in 1720 primarily because he refused to accept the authority of his newly appointed Jesuit superior. While in China Foucquet became a devotee of figurism, the attempt begun by Joachim Bouvet to find basic aspects of the Christian revelation in the ancient Chinese classics. Foucquet hoped figurism would save the Jesuit mission during the Rites Controversy, but his peculiar variety of figurism more often embarrassed both the order and the other figurists. John W. Witek's account of Foucquet's life and thought is thoroughly researched and painstakingly detailed. While it displays the empathy with its subject and familiarity with the inner workings of the society to be expected of a Jesuit, it is not hagiographic. Foucquet was much too controversial for that, and Witek is much too determined to set the story straight. In setting it straight, however, he is quite willing to judge Foucquet's conduct as a fellow Jesuit.

For readers not primarily interested in Foucquet, Witek's book is a mine of information about the Christian mission in China during the early eighteenth century. Both through Foucquet's experiences and in background chapters we can trace some of the political and intellectual trends of the K'ang-hsi period (1662-1723) and the missionaries' attempts to adapt to them. For example, the mission became increasingly dependent on the emperor's protection rather than on the good will of a fairly large number of scholar-officials, as had been the case during the late Ming dynasty. Although Witek does not explicitly say so, the figurists' work reflected this change. They concentrated on the enigmatic *Book of Changes* (*I Ching*), the emperor's favorite classic, treating it as sacred literature and scrutinizing it for figures of Christian truths, whereas earlier Jesuits studying the Confucian classics had looked for general moral principles consonant with Christianity and for differences between ancient and modern Confucianism. Witek also provides a worms-eye view of the damage done to the mission by the Rites Controversy and by the intrusion of the royally sponsored French Jesuits into what had been a Portuguese preserve. His study provides ample detail on the unedifying rivalries between Jesuits and the other orders, between Portuguese and

French Jesuits, between figurists and other Jesuits, and even between Lyonnaise and Parisian Jesuits. No wonder the emperor became confused and impatient when these squabbles surfaced during audiences with the Jesuits and with the papal legates sent to end the Rites Controversy. No reader of these details would be surprised to learn about the eventual proscription of Christianity in China.

Witek's bibliography is exhaustive; it fills 138 pages and will be welcomed by other scholars in the field. The book, however, would have benefited greatly from more rigorous editing.

EDWIN J. VAN KLEY
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JAMES C. COOLEY, JR. *T. F. Wade in China: Pioneer in Global Diplomacy 1842–1882.* (T'oung Pao, *Revue Internationale de Sinologie*, number 11.) Leiden: E. J. Brill. 1981. Pp. 160. f 52.

Thomas Francis Wade, initiator of the Wade-Giles Chinese transliteration system, was one of a very small group of foreigners—mainly British—who survived the rigors of the Chinese environment for a number of decades and who devoted their active careers to various endeavors within China. Wade's career spanned about forty years and combined the roles of erudite sinologue and diplomat. Despite the significant influence that he exerted through both, his life and work have generated remarkably little interest among historians. James C. Cooley, Jr., has produced a somewhat hagiographic and regrettably brief monograph that achieves a rather minor reduction in this void.

He has restructured and elaborated on the thematic sketch of his doctoral dissertation, which is divided in accordance with the two phases of his subject's career. Wade's initial commitment to promotion of a "cooperative policy" (peaceful coexistence *sans* gunboat diplomacy) in the post-1858 treaty settlements was followed by his subsequent (post-1870) formulation of a bicultural diplomacy, which might be supplemented by a discretionary threat or show of force, if and when circumstances warranted. Bicultural diplomacy involved Western concessions to Chinese locked-in preoccupations with formal ritual and ceremony in exchange for Ch'ing accommodation to the principles and practices of the equality-of-all-nations syndrome.

He fills in the canvas by presenting a disjointed historical narrative that focuses narrowly on the principal events and developments that best illustrated Wade's efforts to influence or accomplish the objectives as he perceived them. Thus attention is given to development of direct and tutelary intercourse with high Ch'ing officials consequent on the establishment of permanent embassies in Peking

(1860–64), the abortive Alcock Convention (1868–70), the Tientsin Massacre (1870), the Audience Question (1870–72), China's conflict with Japan over the Liu-Ch'iu Islands (1873–74), and resolution of the Margary Affair, which took place concurrently with the renegotiation of diplomatic and trade relationships through the Chefoo Convention (1875–82). Wade was elevated to the rank of HBM's minister to the Ch'ing court in 1871 and was knighted in 1875.

On the matter of the "cooperative policy" the exegesis is confused because the author does not clearly delineate the different components of the cooperation with which he is dealing. These include, on the one hand, cooperation between Great Britain and those high officials at the Ch'ing court who appeared receptive to "progressive" influences and, on the other hand, efforts among the foreign "Treaty Powers" to effect a united front of support—or of coercion—in their relations with the weakened and vulnerable dynastic authority. The most effective commentary relating to the subject of bicultural diplomacy is the author's own summary. "The subtleties of Wade's approach were usually not perceived by his [diplomatic] colleagues in China or by his superiors in London" (p. 101).

In other respects there are serious drawbacks to the study. It is seriously lacking in depth and breadth of historical perspective. Except where unavoidable, there is only one actor on the stage. Appreciation for the range and complexity of factors affecting attitudes and behavior of other principals in the drama, whether of governments or individuals, is at best casual, at worst superficial. The errors of fact—in dates, place names, footnote citations, and bibliographical entries, many of which are carried over from the dissertation—are too numerous to mention. Portions of the historical narrative, crucial for substantiating a linchpin of the thesis, are undocumented. Many other portions of the text have been inadequately edited, resulting in imprecise and convoluted expression.

There is no question that the author has assembled more information about Wade's career than appears in any other published work. Correspondingly, an outline of Wade's basic understanding of Sino-Confucian society and of his commitment to try to assist in its transformation, despite periods of intense frustration and despair, does emerge. It is to be hoped that others may be stimulated to undertake a more intensive and comprehensive investigation of the career of this rather remarkable man.

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MICHAEL R. GODLEY. *The Mandarin-Capitalists from Nanyang: Overseas Chinese Enterprise in the Moderniza-*

tion of China, 1893–1911. (Cambridge Studies in Chinese History, Literature, and Institutions.) New York: Cambridge University Press. 1981. Pp. viii, 222. \$39.50.

In this book, Michael R. Godley tries to fill what he regards as a gap in the literature on the modernization process of China in the late Ch'ing period. He attempts to examine how the overseas Chinese in Southeast Asia were recruited by the Ch'ing government to participate in the Chinese bureaucracy and what role they played in China's economic development. His method is principally to trace the career of Chang Pi-shih (also known as Thio Thiau Siat, 1840–1916), the foremost overseas Chinese entrepreneur in Nanyang (South Seas).

Godley notes that as a group the overseas Chinese in Nanyang achieved an economic pre-eminence far out of proportion to their numbers. Through frugality and industry, they became not only hawkers and tradesmen but also first-rate entrepreneurs. But they were never accepted by the European ruling elite; they had to seek social recognition in their homeland. Besides, they needed high status for protection at home. As Godley points out, they were not rebel sympathizers participating in the overthrow of the Ch'ing dynasty, as commonly believed; they were its supporters. The Ch'ing government, in turn, shaped a policy designed to attract their capital and skill by granting them official ranks and honors and a major part in economic affairs.

To illustrate all these points, Godley describes in great detail the career of Chang Pi-shih. Born in rural Kwangtung in 1840 and leaving home for Southeast Asia at the age of seventeen, Chang began as a provisioner for the Dutch army and navy in Java. Twenty years later he became one of the most prominent Chinese businessmen in the area with dominant investments in shipping, mining, banking, and large plantations. Later in 1895–1910 he extended his interest to China, establishing businesses in a wide variety of fields, including wine-making, textiles, brick-making, glass-making, salt, cattle, mining, and cotton. He was also associated with railroad projects in Kwangtung and Fukien and was the largest private stockholder of the Imperial Bank of China.

Chang was first recruited by Ch'ing officials (Hsueh Fu-ch'eng and Huang Tsun-hsien) into the service of the Ch'ing empire in 1893 when he was appointed as vice-consul at the Chinese consulate in Penang. He was eventually made a vice-president of the newly founded Board of Trade, having been granted many imperial audiences and official rank. He was assigned a decisive role in bringing Nanyang Chinese into participating in China's development.

In telling Chang's story, Godley uses a variety of primary source materials, especially local newspa-

pers. Undoubtedly, Godley digs out a great deal of useful information (his detailed account of the personalities and events involved does not make the book easy reading), but Godley makes no attempt to place those personalities and events in a broader perspective. How much investment was made in China by the overseas Chinese from Southeast Asia? Was it actually very small? Why so small? How important was their participation in the overall development of the Chinese economy? How profitable were the Nanyang investments in China? Did they encounter any particular obstacles that were not encountered by Chinese domestic or foreign businessmen? All these and related questions need to be answered before one can have an overall judgment on the role of "overseas Chinese enterprise in the modernization of China, 1893–1911," which is the subtitle of the book.

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SUE GRONEWOLD. *Beautiful Merchandise: Prostitution in China, 1860–1936.* (Women and History, number 1.) New York: Institute for Research in History or Haworth Press. 1982. Pp. x, 114. \$20.00.

Sue Gronewold's book is a study of prostitution in China. It begins with an outline of prostitution before 1860: its typology, organization, recruitment patterns, market prices, and the population of prostitutes. In the following chapter, Gronewold argues that, while the Chinese law viewed prostitutes as "mean" nonpersons, their convention saw them as condemnable and local attitudes were usually lenient toward prostitution. Gronewold also maintains that Chinese women were marginal to society, and they were treated as disposable merchandise. In substantiating her argument, Gronewold examines various means of mistreating women such as infanticide, footbinding, and sale of women as prostitutes. In times of economic hardship or famines, such mistreatment was more widespread.

According to Gronewold, a number of changes occurred on the societal level between 1911 and 1936 that might have contributed to the expansion and changing nature of prostitution. These changes included the struggle for power among the militarists (warlords), the increasing immiseration of the rural population, the rise of a new Westernized elite, and the introduction of Western "modernity." Gronewold argues that, although there were some reforms designed to eliminate prostitution in the first few decades of this century, these government reforms were largely useless because the governments needed to exact revenue from brothels and the law enforcers, namely the police and local officials, were corrupt. She concludes that it was not

until the Chinese revolution of 1949 that prostitution was removed.

Chinese prostitution is a relatively unexplored area in historical research largely because of its stigmatized nature and the unavailability of literature. Gronewold's greatest contribution is therefore her detailed account of the types and organizations of prostitutes before 1936. Another contribution of Gronewold's book lies in her attempt to locate prostitution in the changing political and economic context. She insists that the family economy as well as the larger economic structure placed women in a peripheral position, and prostitution was merely an indication of women's marginality. Gronewold also breaks some new ground in showing class differences among prostitutes. However, due to her sources of information—writings of the literate elite, Western travel manuscripts, novels, and paintings—her description of prostitutes serving the elite appears to be more detailed than that of poor prostitutes.

There are a number of problems in Gronewold's book. First is her vague definition of what prostitution is. She includes sing-song girls and entertainers in the same category as women who received money for sex. With such a broad definition, the linkages she tries to draw between prostitution and economy are rendered meaningless. At times, moreover, it is not clear what theses Gronewold is attempting to present on the relationship between the economy and prostitution. Does poverty *per se* lead to prostitution, or does the escalation in economic deterioration contribute to the expansion of prostitution? The historical period that Gronewold chooses for examination was one of continuous economic deterioration (except for only a few minor upturns, especially during the First World War). Methodologically, it does not provide a base for comparison. In order to strengthen her argument she needs to locate a historical period in China in which economic prosperity was the norm. Or alternatively, she needs to find two regions in China—one relatively rich, the other poor—and compare the situation of prostitution in these two areas.

Gronewold covers a large time span—seventy-six years—in her analysis. On the political level, the period between 1860 and 1936 witnessed the development of the traditional Ch'ing economy and the economies of the republic, the warlords, the Kuomintang, and the Communists. One would have expected Gronewold to make full use of this diversity to examine the relationship between the nature and extent of prostitution and the political structures. Unfortunately, such expectation ends largely unfulfilled. Furthermore, it is still unclear why Gronewold brings into the discussion the "superstructure" (law, convention, belief, attitudes, and so forth) of prostitution. After devoting one full chap-

ter of her book to this topic, Gronewold does not attempt to connect the "superstructure" with the "sexual economy" or the "economics of sexuality." In what ways does the "superstructure" affect the nature and extent of prostitution, and how does it relate to the psychology of the prostitutes themselves?

Finally, one important social phenomenon has been missing in Gronewold's book, and that is the antiprostitution efforts within the Chinese women's movement. Toward the end of her analysis, Gronewold discusses the changing values on sex and the government's moralistic gestures in eliminating prostitution after 1911. These sections do not capture the essence of early missionary attempts to fight against prostitution and the Chinese women's organized efforts to stamp out prostitution during the May Fourth period and the early 1930s. Such neglect on the part of the author appears to strengthen the book's image of Chinese women as victims of society and to undermine their role as agents of social change. This is not an accurate picture of Chinese women during the historical period under study.

BOBBY SIU

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EUGENE LUBOT. *Liberalism in an Illiberal Age: New Culture Liberals in Republican China, 1919–1937*. (Contributions in Intercultural and Comparative Studies, number 5.) Westport, Conn.: Greenwood Press. 1982. Pp. 194. \$27.50.

Eugene Lubot's book is a broad-brush sketch of the relation of Chinese liberals to politics in the 1920s and 1930s. Lubot convincingly argues that New Culture liberals were, by stages, politicized by the pressures of the period in which they lived. Like Hu Shih, who "vowed not to talk politics for twenty years" (p. 33), most began in the 1910s with the conviction that the revolution China most needed was cultural in nature, and therefore they eschewed political action in favor of educational work and public critiques of the negative aspects of traditional Chinese culture. In the second stage, the early 1920s, however, most liberals began to act as political commentators, while still rejecting direct involvement in political organizations (except for a brief participation in the "Good Men Government" scheme under President Li Yuan-hung in 1922). The abrupt and disappointing end of the Good Men Government sent liberals in several directions in the 1920s, though most still avoided direct involvement in political organizations. During the third stage, from 1927 through 1933, a few liberals embraced and participated in the Kuomintang (KMT) and its government, while most others came

to criticize vehemently the regime for its increasingly stiff censorship and lamentable record on human rights. The fourth period, from the T'ang-ku Truce through the Japanese invasion of China proper saw a nationalistically inspired gravitation by many liberals toward the government in response to Japanese military and diplomatic pressure on China.

Lubot's work is a welcome addition to the growing body of writing on the complex texture of the KMT regime's relationship with the society it tried to govern. On the one hand, with few exceptions liberals were not part of the political base of the KMT. On the other hand, the growing affinity Lubot notes between the stated political positions of many key liberals and the Kuomintang (even to the point that several embraced the idea of dictatorship) helps amend the notion found in some recent scholarship to the effect that the KMT was unreflective of the views of any segment of society.

Still, this book does not drastically modify the overall view of Chinese liberalism available in the works of Jerome B. Greider, Charlotte Furth, and others. Its significance lies in the fact that it ably tracks a wider cross section of liberals than do most scholars; well over two dozen Chinese liberals dance across the canvas Lubot paints. Thus, when he identifies trends among liberals, we may be confident that he is not wildly overgeneralizing from too puny a sampling.

By far the least satisfactory part of the book is its conclusion. There Lubot suddenly and inexplicably tries his hand at comparative history, digressing at length on nineteenth-century British liberalism. By pointing out British liberals' naiveté and their gross underestimation of the irreconcilability of English class interests and social divisions, he tries to demonstrate that Chinese liberals, despite their belief that they could promote liberalism in an age of repression and violence, were not uniquely naive. His comparison with Britain is forced and not very illuminating. Certainly liberal values were more widely shared in Britain than in China. More apt comparisons could be drawn between early twentieth-century China and almost any other developing nation (tsarist Russia immediately comes to mind). And if cross-national comparison was to be included, it should have been thoroughly integrated into earlier chapters.

Additionally, one could fault the author for his scanty exploration of the emotional and intellectual dimensions of the acceptance of violent political solutions by people he identifies as liberals, like Tuan Hsi-p'eng and Ts'ai Yuan-p'ei, both of whom played roles in the bloody anticommunist purge. In fact, though Lubot allows that Tuan "abandoned his conventional liberal ambition" of teaching, one might even question whether Tuan was a liberal at all. Or perhaps the time has come to re-evaluate

completely use of labels like "liberal" and "radical" in the context of modern China.

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MARY ELIZABETH BERRY. *Hideyoshi*. (Harvard East Asian Series, number 97.) Cambridge: Harvard University Press. 1982. Pp. xiv, 293. \$30.00.

Toyotomi Hideyoshi, the former peasant who united all Japan under his rule in 1590, was one of the great men of Japanese history. Few of us who write of Japan in the sixteenth century can avoid invoking his name on occasion, but, until the appearance of Mary Elizabeth Berry's *Hideyoshi*, no one writing in English had produced a sustained scholarly treatment of the man's career.

In Berry's carefully crafted study, the first sixty-five pages of text provide the historical background for Hideyoshi's activities; the next one hundred treat his unification of Japan between 1582 and 1590, and the last seventy explore both his "pursuit of legitimacy" and his seemingly lunatic behavior at the height of his power. Throughout, Berry maintains an essentially Kyoto-centered perspective on the events of the sixteenth century, and she argues that Hideyoshi's personal role in the creation of Japan's early modern polity was enormous.

This is a well-written and provocative book, and it will be read with profit and pleasure by specialist and nonspecialist alike. Indeed, it is must reading for anyone seeking to understand Japan's medieval-early modern transformation. I myself do not agree with everything Berry says; but then hers is a pioneering venture into poorly mapped territory, and a certain amount of quibbling is to be expected.

I do wish to make two observations. The first concerns the limitations inherent in viewing a history as complex as that of sixteenth-century Japan from the perspective of an individual life. For example, the question of the precise social and political impact of Hideyoshi's cadastral surveys is a thorny one, and one on which a great deal of fundamental research remains to be done. While the complexities of this and similar questions are not always relevant to the story of Hideyoshi the man, it is probably true that a full understanding of the history of sixteenth-century Japan must await the answers to questions of a more elusive sort than Berry has needed to pursue here.

Secondly, I wonder whether Hideyoshi's pursuit of legitimacy may not have been slightly more complex than Berry suggests. I am certainly convinced that both the radically monarchical policies of Nobunaga and the moribund shogunal model of the Ashikaga were foreclosed to Hideyoshi, and I doubt that anyone ever regarded the "feudal con-

tract" as a comprehensive model for rule at any level. While I am therefore not at all surprised that Hideyoshi found it necessary to posture as the dutiful minister to his emperor, I wonder whether the ideological possibilities of medieval lordship were not rather richer than we have hitherto realized. I have no wish to attack the particular views that Berry advances, but I suspect that she herself would concede that the issue remains open.

The openness of a question, however, does not belie the significance of a study that has either grappled with or given rise to it. Berry's is an extremely useful book, and one that deserves a wide readership.

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JEAN-PIERRE LEHMANN. *The Roots of Modern Japan*. New York: St. Martin's Press. 1982. Pp. xviii, 352. \$26.00.

This is a disappointing book, of uneven quality, with an exceedingly ambitious goal that is not quite fulfilled.

Jean-Pierre Lehmann, Director of the Centre of Japanese Studies at the University of Stirling in Scotland, sees Japan, a paragon of social stability and national cohesion, as a dynamic alternative at a time when Western-style capitalism faces its worst crisis since the Great Depression and Eastern European socialism seems manifestly bankrupt. Hence he tries to analyze the roots of present-day Japan by focusing on the period from the seventeenth to the twentieth centuries, during which, he believes, the ultimate and significant patterns of the Japanese nation were formed. This approach, he argues, not only will explain the Japanese revolution, whose effects, according to him, are more profound than those of the French, Russian, or Chinese revolutions (a captive of one's own parochialism?), but will also put the Japanese experience into a universal perspective, whatever that means.

Confidence in the work is not inspired by the number of errors one encounters. A few examples should suffice. On page 11 he refers to the Mughal (Persian) dynasty, on page 15 to Prince Shih of the Ch'in dynasty. On page 18 one is informed that primogeniture existed in Japan long before Ashikaga times; on page 20 we learn that Saigo Takamori was killed by one of his kinsmen. On page 33 Magellan sets sail in an easterly direction. On page 37 Columbus turns out to have been a Genoese Jew, Will Adams arrives in Japan aboard a vessel called "Erasmus," while St. Francis Xavier is made to die in Goa (quite a page). On page 297 (to skip a lot of errors in between) it is stated that only Russia had a direct interest in the Triple Intervention of 1895.

The style, too, sometimes is obtuse, and some subjects are treated with much condescension. What is one to make of the following, for instance: "printing . . . was an activity of quasi-febrile dimensions in Edo Japan" (p. 109), or "culture, as such, was by no means new to Edo Japan" (p. 110). One of my favorites deals with gunpowder and the printing press, both of which "as every schoolchild knows, were Chinese inventions, but for reasons uncertain, having invented them, the Chinese then forgot about them" (p. 41).

There are, on the other hand, some very positive contributions made by Lehmann. His general thesis, that Japanese history is evolutionary and that the dualistic approach of traditional versus modern holds not true, is well substantiated. Japan is successful today because the study of external models and a certain institutional and social radicalism brought vitality to its society and led to successful institutional innovation. The author also, quite rightly, points out that Japan cannot be emulated, since its society is a reflection of a particular culture that cannot be transplanted into a different environment. He quotes Levi-Strauss to the effect that Japan is the only country that has succeeded in modernizing without betraying its origins. Another valuable point made is the fact that Japan, though now a world power associated with the West, is geographically, racially, and culturally part of the East Asian world—and suffers from this ambivalence. He sees this situation as producing constant anxiety and resulting in an unending quest for national identity.

Especially valuable and very well done indeed is the solid treatment given in individual chapters to such affairs as the importance of Rangaku and the special relationships established between state and private enterprise. In chapter 7 the sections on bureaucracy, workers and entrepreneurs, and peasants and landlords are very fine. Chapter 8, dealing with the making of the Meiji state, describes, in excellent fashion, the rise of militant nationalism, education and the state, and the introduction of Western law.

In summary, despite irritating errors, there is a lot of substance in individual chapters, but the basic problem is a feeling of disappointment, since none of the trends developed (as roots) are ever applied to an analysis of post-1945 Japan. If we cannot understand modern Japan without knowing its roots, how can we know whether Lehmann is right in describing the roots without looking at modern, postindustrial Japan?

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TETSUO NAJITA and J. VICTOR KOSCHMANN, editors.
Conflict in Modern Japanese History: The Neglected

Tradition. Princeton: Princeton University Press. 1982. Pp. x, 456. Cloth \$42.50, paper \$14.95.

This provocative work probes conflictual events in order to discover their broader significance. Scholars are well acquainted with the urban riots, assassinations, coups, and other, similar conflictual events that fill modern Japan's historical landscape, but, according to the editors, these events are viewed as aberrations and misinterpreted. While not denying the importance of consensuality, the authors feel that conflicts are clustered around discontinuous and synchronic systems. The editors modestly add that this framework is provisional and requires additional refinement.

Revisionist historians have challenged the consensus theory for some time, but this book adds another element: a repudiation of the historian's identification of history with linear time. Tetsuo Najita argues that this traditional approach tends to interfere with the aim of achieving historical understanding. A more effective method of analysis is the "synchronic" (that is "simultaneous time" or "simultaneous experience"). Using this method of "thick history," these essays grapple with the theme of conflict as a central Japanese experience. Some historians, no doubt, will be surprised at Najita's call for a "synchronic" approach, because he is urging the adoption of French structuralists' methods together with those of "scientific history," just as these methods are being rejected as inadequate.

The strength of this book is the individual essays, all of which are carefully researched and intelligently written. Harry Harootunian persuasively argues that an attempt to overcome an epistemological break that occurred during the Tokugawa era generated conflict. Perceptions of discontent, he writes, were grounded in and sanctioned by new epistemological possibilities generated by eighteenth-century thinkers. Conrad Totman deftly spotlights the political crisis at the end of the Tokugawa period, illustrating how officials sought to regain political control by abandoning the old order in favor of a newly fashioned one. J. Victor Koschmann skillfully analyzes the *Tengu* Insurrection in order to explore the symbolic dimensions and structural features of conflict throughout society. The role of *shishi* (men of high purpose) in bringing about political change is nicely covered by Thomas Huber. *Shishi* deserve, he feels, the pivotal importance assigned them by Japanese scholars, because value conflict within their ranks (that is sentimental loyalism versus pragmatism) became a major legacy for Meiji leaders.

M. William Steele's interesting essay centers on the "principles of action" that drove the supporters of the failing Tokugawa regime, by tracing the rise and fall of the *Shōgitai* (League to Demonstrate Righteousness). Hashimoto Mitsuru sees behind the

peasant movements and uprisings a reaction to the political and economic disturbances of the times: by attacking local symbols of power and wealth, peasants were notifying authorities that they wanted an end to current confusion and a return to the "good old days." Stephen Vlastos's interesting contribution is on *yonaoshi* ("world rectification" or "world renewal") rebellions in Aizu. The overriding concern of those who took part in these sometimes violent attacks on wealthier people was an acute fear that political change would deprive them of their land. Part 1 of the book is neatly concluded by George Wilson's chapter on pursuing the millennium. Japan's crisis provoked various people to take drastic action in order to fulfill their hopes. Wilson points out that these people were driven by timeless, powerful myths. A mixture of tradition and innovation is emblematic of millenarian movements in general, and it became a hallmark of Japan after 1868, concludes Wilson.

Oka Yoshitake opens part 2 with a brilliant essay on the origins of Taisho Democracy, which he finds in the growth of an individualistic way of thinking after the Russo-Japanese War. Coupled with growing self-awareness on the part of youth was a waning of loyalty to the state. A new consciousness of the individual among youth strongly challenged national consensus, which led to generational conflict with the directive elite. Bernard Silberman's fine essay is on the problem of legitimacy and authority in a bureaucratic state. All revolutionary regimes, if they wish to retain power, are forced to create a state bureaucracy to solve problems that arise out of the revolutionary act. This solution, however, ultimately leads to conflict in which rules and the positive law become the weapons. Bureaucrats reply to conflict with a further rationalization of society. The Hibiya Riot of 1905 is skillfully dissected by Shumpei Okamoto who repudiates the view that the riot was a popular political upsurge against the ruling clique. "Dominating the mind of the crowd was not political reform but simple and direct opposition to those who were 'criminals' against the emperor and themselves" (p. 272). Higher education, too, was filled with conflict, says Byron Marshall. His stimulating essay illustrates that growth and change eroded old ties between bureaucratic and intellectual elites. During Taisho this conflict grew sharper and led to a crisis in governance; the older style of mediating disputes via the education ministry was replaced by justice ministry criminal proceedings. James Bartholomew's excellent contribution opens a door on the neglected area of science, by following the career of Kitasato Shibasaburō who had extraordinary success in using factional ties and politics to maneuver and retain institutional autonomy.

Ron Napier and Ann Waswo investigate aspects of labor conditions. Napier ably shows that labor's

share of national income improved after 1917. Tenant disputes and urban strikes reflected labor's attempt "to actualize" its new position. Waswo's look at tenant unions is fascinating. As interesting as the conflictual behavior between government officials and landlords on one side and union leaders on the other is the equally conflictual relationship between tenant unions. The reaction of liberal intellectuals during Taisho to social disintegration and conflict is vividly portrayed by Peter Duus. They turned to the Diet as the mechanism to achieve collective harmony, but "the liberal goal of a harmonious parliamentary state was no more attainable than the iron cage of the bureaucrat. Both rested on the illusion that a consensual society was possible" (p. 439).

The difficulties in editing so diverse a collection are obvious in the epilogue done by Koschmann. It appears that the authors were in full agreement on only the fact that a new methodology might fruitfully be applied to the study of modern Japan. The essays "adamantly resist full integration" and we are left, writes Koschmann, "with a number of 'interpretations of conflict.'" Yet when placed together these fragments "form a whole, of a sort, by virtue of their placement in two encompassing compositions" (p. 442).

While I am unconvinced by the case made for a new historical methodology and by Koschmann's conception of what forms a "whole," the editors are to be congratulated for numerous insightful and intriguing comments. They, together with other contributors, have produced a collection of indispensable essays.

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ROGER BUCKLEY. *Occupation Diplomacy: Britain, the United States, and Japan, 1945-1952*. (International Studies.) New York: Cambridge University Press. 1982. Pp. x, 294. \$39.50.

Roger Buckley has produced the first thorough examination of British policy toward the occupation of Japan between 1945 and 1952. It is based on a solid knowledge of the relevant archives in the Public Record Office in London, and the author has consulted various American collections, including the papers of MacArthur and Dulles. Attention is focused on certain key themes: the nature of British wartime planning for handling Japan after surrender; diplomacy toward the early stages of Allied control; attitudes toward Emperor Hirohito and constitutional arrangements and toward various political, military, and legal issues; the formulation of British economic objectives; relations within the British Commonwealth concerning Japan; and an

interim assessment of exchanges culminating in the San Francisco Conference in 1951. Buckley demonstrates that the extent of British interest in Japan was greater than previously believed and that strenuous efforts were made to influence American policy making at various phases of the occupation, notably in 1945-46 when the Attlee government hoped to secure an effective voice in determining policy in Tokyo and between 1947 and 1951 with reference to discussing the proposed peace treaty. No deep impact, however, was made on American policy makers as a result of declining British power and of the complex relationship between General MacArthur and the competing elements of the Washington bureaucracy. The position of MacArthur is central to most of this study, that is, down to his dismissal by President Truman in April 1951. The author states that "General MacArthur was neither an anglophobe nor an anglophile" (p. 32), and this is a fair observation. MacArthur was less hostile toward Britain than has sometimes been contended. He frequently met the principal British representatives in Tokyo, General Sir Charles Gairdner and Sir Alvary Gascoigne, and their exchanges were mostly of an amicable nature. MacArthur professed much admiration for the spirit and resilience of the British people and spoke warmly of Ernest Bevin's work as foreign secretary. All who met MacArthur, however, appreciated his dramatic talents and were never certain how far the image corresponded with the reality. In the later stages of the occupation Gascoigne's meetings with MacArthur became less common, and more friction occurred.

British opinion of MacArthur was in the main extremely favorable: officials and visitors spoke warmly of the general's success in persuading the Japanese people to accept change under American guidance and of MacArthur's avuncular qualities. Indeed, Whitehall found it difficult to envisage the American presence in Japan without him and concern was expressed (prior to June 1950) whenever it was rumored that he might be replaced. Britain supported the retention of the emperor and the reform of institutions, if with skepticism as to what would be accomplished by naive American reformers. It was held in London that the occupation should not be unduly prolonged, and consistent pressure was exerted for an early peace treaty. The attempt to secure this in 1947 failed, in part because the British exaggerated the extent to which MacArthur could influence Washington. Vested interests in Britain, particularly in the Lancashire textile industry, sought to restrict aspects of Japan's economic revival. Bevin was critical of such attempts, although the Board of Trade under Harold Wilson proved more responsive (pp. 168-69). The degree of tension in Anglo-Australian relations is clarified:

this was the outcome of growing Australian dependence on the United States for defense purposes, accentuated by the volatile character of Herbert Evatt. The final chapter is inevitably of a provisional nature, as the archives were not available when the work was completed. This is a valuable, informative study and will do much to improve understanding of the development of British and American policies in East Asia after 1945.

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BARBARA DALY METCALF. *Islamic Revival in British India: Deoband, 1860–1900*. Princeton: Princeton University Press. 1982. Pp. xiv, 386. \$27.50.

Barbara Daly Metcalf's book, a revised version of her doctoral dissertation, sets out to study the response of the Muslim ulemas to the changed situation created by the imposition of British rule in India. The collapse of Muslim rule was catastrophic for the Indian ulemas as it deprived them not only of their patrons but also of their position in society and their traditional role as advisers and guides to the ruling classes.

The author challenges the common assumption that Islam stagnated in the nineteenth century and that all reformist movements were linked with the adoption of European ideas and learning. While she does not deny the importance of Western influence, she argues that all religious "change in this period primarily entailed self-conscious reassessment of what was deemed authentic religion—it was not syncretism, not acculturation to western patterns, not conversion" (p. 348). The Muslim reformers were the products of Islamic tradition, operating within the framework of Islamic code. They were not "alienated" or "marginal" Muslims.

Unlike several other works on similar themes, the author emphasizes the need of seeing the Deoband movement according to the perception of the participants, and the result is quite fascinating. After providing a panoramic view of the various Islamic reform movements in the eighteenth century, Metcalf devotes four large chapters to a detailed but lucid account of the establishment of the Deoband Madrasah, its administration and finance, and the education and training of young scholars there. The Deoband scholars sought a renewal of the spiritual life of Indian Muslims by preaching a return to the pristine purity of Islam and correct interpretation of Quran and Hadis. Their influence, both among their contemporaries and also in subsequent years, was quite widespread. The reforming ulemas, borrowing a technique of the Christian missionaries, made extensive use of the lithographic press to publish Qurans, Hadis, and learned commentaries

on Islamic subjects. These were widely circulated during the period. According to a government estimate, nearly twenty-three thousand copies of the Quran were distributed in 1871 alone. Most of the commentaries, together with translations of the Quran, were published in Urdu, which thus acquired the status of the language of Muslims. A curious by-product of the use of Urdu by the reformist ulemas was the opposition it aroused among the Hindus, who now pressed the claim of Hindi as the national language. Ironically for the subsequent development of the Indian nationalist movement, many minor Hindu officials, in order to establish the predominance of Hindi, began to push its claim as the language of the majority of the population and thus unwittingly gave Urdu a sanctity among Muslims even in regions like Bengal where Urdu was scarcely spoken by the vast majority of Muslims.

The last two chapters do not deal with the Deoband school but discuss other alternative movements of renewal: the Ahl-i-Hadis and the Barelvis, Aligarh and Nadwah. Although the contents of these chapters appear to be outside the scope of the book (or else they might have been integrated in the main narrative) and do not provide any fresh interpretation, they contain a most readable account. The piece on the Aligarh movement is a particularly useful synthesis of more recent research work.

Metcalf exploits her skill in Urdu by making extensive use of biographies and hagiographies, diaries, memoirs, and tracts. She not only provides thereby a wealth of fresh information but also succeeds in mirroring the minds of the ulemas. The work, at times, is slightly marred by somewhat uncritical acceptance, even if only implicitly, of her source material. One example of this is her ready acceptance of the Deoband view of the Shi'as as heretics. On page 152, as in several other places, she reports without a demur: "They held the Shi'ah deny the singularity of God, the humanity of the Prophet, and the finality of revelation." Surely such a statement requires careful scrutiny. In a book that sees the work of reformist ulemas as a reaction to alien rule, it would have been worthwhile to examine Shi'a attitudes in greater depth. The fact that current upheaval in the Islamic world is more pronounced in areas with Shi'a populations raises questions crucial to any understanding of Islamic reformist movements.

This book is a useful contribution to the subject and a cogent reminder that whether one is studying the Wahabi movement in the eighteenth century or the more recent ferment in Iran, Islamic movements cannot be slotted into preconceived pigeon-holes or explained by such simple terms as primary resistance, rebellion, social reform movements, obscurantism, or reactionary religious fanaticism—

phrases so popular among modern analysts. The Islamic movement must be understood on its own terms. Account must be taken of the perspective of the participants, their faith and belief, their view of life and afterlife, of God and the finality of the Prophet Muhammad, of Quran and Hadis. This is what Metcalf has tried to accomplish so admirably.

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PAUL R. GREENOUGH. *Prosperity and Misery in Modern Bengal: The Famine of 1943–1944*. New York: Oxford University Press. 1982. Pp. xvii, 342. \$37.00.

The Bengal famine of 1943–44 was one of the worst catastrophes of modern Indian history, with excess deaths over normal mortality numbering perhaps three million. More sophisticated efforts to analyze the famine have been appearing in recent years, including the writings of the eminent economist Amartya Sen and now this excellent study by historian Paul R. Greenough.

The latter argues that this famine (but by implication others as well) “was culturally patterned in its onset, crisis, and denouement” (p. 265). It was culturally patterned because the Bengalis had their own way of understanding prosperity and of dealing with the crisis of famine. He shows how rice cultivation, consumption, and distribution are central to Bengali society and how relationships between rice providers and rice receivers have been culturally shaped.

In the wartime situation of 1942, with the Japanese on the Indo-Burmese frontier, British officials sanctioned a boat-denial, then a food-denial policy. But Greenough argues that the famine was caused by the disruption of the traditional marketing system, followed by a growing food shortage, fear, widespread hoarding, and only feeble efforts to control the market.

Using records of the Bengal Relief Committee, other contemporary accounts, and interviews with survivors in 1979, he has analyzed the victims of the famine to demonstrate that for important cultural reasons—namely, the long-term survival of their culture—Bengalis sacrificed children and the elderly but tried their best to preserve adults in the twenty-to-forty age range because they could replicate the society if they were the only survivors. This argument is skillfully presented and bodes well for what the author calls his “modified ‘substantivist’ approach” to the study of famines.

He claims that he has united this approach to some extent with what he calls the nationalist approach, which places significant blame for the disaster on the foreign rulers. His bias against this latter viewpoint and his scanty knowledge of the politics of

the period, however, lead to a neglect of the political context and of important events of the period. He argues that in such a complex situation it is impossible and simple-minded to assign blame as many Indian writers have done. But some people in positions of power make decisions of wide import and must bear more responsibility for failures as well as successes.

In defense of those who believe that a political dimension is a necessary part of the analysis of the 1943 famine, it should first be noted that in the period from 1940 to 1943 Bengal had one of its worst governors (Sir John Herbert, a minor county politician at home, at sea in administering complex matters) and that India had a narrow and rigid viceroy, Lord Linlithgow. When the new viceroy, Lord Wavell, came into office in October 1943, he was appalled that his predecessor had never paid close attention to the famine. Wavell immediately visited Calcutta and made it clear that dealing with the famine was a national concern. The next governor, Lord Richard Casey, also was a more experienced and able administrator.

Greenough tells us that 1941 “saw no great changes politically” (p. 87), but, in fact, there was an important change of ministries in December 1941 with the fall of the Muslim League government and the establishment of the Progressive Coalition. The latter was an effort at Hindu-Muslim alliance that did not please Jinnah or the British, and they made every effort to weaken this government. This political conflict and weak administration at the upper levels of the Raj was complemented by the appointment of L. G. Pinnell, ICS as Director of the Civil Supplies Department, a role for which he was unsuited and in which he made several foolish choices. The political and administrative setting allowed profiteers, including the well-known Ispahani family, to take advantage and reap large gains.

These political matters are scarcely touched by Greenough, who has other concerns. It would be valuable for another historian to dig into these, while remembering that Greenough has put us in his debt with a fine cultural, social, and economic analysis of the famine, the best account I have read to date.

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DWIJENDRA TRIPATHI. *The Dynamics of a Tradition: Kasturbhai Lalbhai and His Entrepreneurship*. New Delhi: Manohar Publications. 1981. Pp. x, 243. Rs. 50.

Kasturbhai Lalbhai (1894–1980) came of a family of Oswal Jains whose ancestors had been prominent in the commercial life of Ahmedabad for generations.

In 1905 his father launched the Raipur Mills in Ahmedabad, a city in western India where the cotton industry was already well established. Kasturbhai effectively took over the operation of the enterprise on his father's untimely death in 1912. Profiting from the wartime demand for cotton goods and successfully meeting the challenge of fluctuating markets in the interwar years, he expanded his business by founding new mills and installing various relatives in managerial positions in them. Just before and during the Second World War, he diversified into the manufacture of starch and sulphuric acid, while consolidating his textile ventures. At the end of the war, he began producing synthetic dyestuffs and pharmaceuticals in collaboration with American Cyanamid. He remained in active management of his various concerns, all of which remained confined to his home state of Gujarat, until his retirement in 1977 at the age of eighty-three. Meanwhile, he pursued a life of public service (for example, as a member of the Central Legislative Council and as a director of the Reserve Bank of India) and continued the family tradition of generous contributions to public charities.

Such, in outline, was the career of the subject of this book. Dwijendra Tripathi tells this story reasonably well though quite uncritically. Yet at the end, we are left wondering why Kasturbhai is not in the same class as some of the great Indian entrepreneurs who were his contemporaries, J. N. Tata and G. D. Birla, for example. This sense of uncertainty stems from the weakness of Tripathi's analytical framework. As the subtitle indicates, this is a study in "entrepreneurship," but the brief discussion of what constitutes an entrepreneur and entrepreneurship is unsatisfactory. Schumpeter's and Gerschenkron's names are dropped in the introduction; Weber, Hagen, and McClelland are touched on in the conclusion. But there is no serious attempt to relate any general theory of entrepreneurship to the specifics of this particular case. Instead, Tripathi puts forward what he calls a new concept of "constellation of forces" (meaning simply all the external and internal influences that bear on an individual and an enterprise) in an attempt to explain such questions as why Kasturbhai refused to expand his business operations outside Gujarat, though he had the financial resources to do so, and why his management structure remained paternalistic and centralized to near the end of his career. Was Kasturbhai no more than just a good businessman who made sound (or lucky) decisions? What were the qualities he lacked and the Tatas and Birlas possessed that made the latter the equals of any of the merchant princes of Europe and America? This book does not provide the answers to such questions.

Another weakness of the book is the author's

repudiation of the historian's necessary practice of footnoting his sources on the grounds that they are neither easily accessible to the "reading public" nor amenable to footnoting without needless repetition. This raises the question of what audience the book is aimed at. Since the foreword states that the book is part of an effort by the Indian Institute of Management at Ahmedabad, where Tripathi is a professor, to promote research in Indian entrepreneurial history, one must assume that the prime target is academic. In that case, the academic reader, well used to the tedium of footnotes (having made his own contribution in that area in his time), will accept the burden in return for detailed knowledge of the range of sources and their exact use. As this book lacks both a satisfactory theoretical framework and a sufficient scholarly apparatus, the best that can be said for it is that it is a useful empirical contribution to the greatly neglected area of Indian business history. But what a chance has been missed!

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ROB NIEUWENHUYNS. *Mirror of the Indies: A History of Dutch Colonial Literature*. (Library of the Indies.) Translated by FRANS VAN ROSEVELT. Edited by E. M. BEEKMAN. Amherst: University of Massachusetts Press. 1982. Pp. xxix, 336. \$27.50.

The number of historians in the United States who will be immediately motivated to read this book is very small; the number who would profit from it is very large. By means of a wide-ranging, unpretentious, and very shrewd overview of the literature of the Dutch in Indonesia, it opens up the cultural dimensions of one of the longest, most complex, and least known (outside the Netherlands and Indonesia) of all colonial dominations. Students of other empires, other cross-cultural dominations, will find in it much food for thought. The series editor, in his introduction, even suggests affinities between the literature surveyed and that of the American South. That seems to be reaching a bit, but it would be very interesting to have the reactions of a historian of the culture of the American South to this book. The student of India will be able to draw many fascinating comparisons between the Dutch colonial literature and the literature of the English in India from Hickey to Forster to Jhabvala.

This book is a translation and abridgement of the author's *Oost-Indische Spiegel* (1972; 3d ed., 1978). Rob Nieuwenhuys writes as a participant in the last phase of Dutch rule in Indonesia and a contributor to its literature. This gives his book a chatty, "insider" quality that occasionally is a little disorienting to the novice reader, but in the long run it contributes

greatly to his interpretations and evaluations of the literature and to his frequent insights into its social and cultural backgrounds. Sometimes even the author's analyses of style are important to the cultural historian, as in the descriptions of J. Olivier's shifts from discursive calm to rancorous satire and of A. Alberts's blending of a laconic, almost surrealist style with a sense of the overwhelming strangeness of the natural world of Indonesia.

The cultural historian also will find in Nieuwenhuys's many summaries of the lives of authors and the plots of their books a number of recurring themes: the new emigrant who cannot find a place either in the tightly hierarchical colonial establishment or in native society, the man who came east to get rich and ends his days on an isolated plantation miles from the nearest European, and the honest but very tenuous Dutch efforts to build Dutch-Indonesian cultural understanding between 1900 and 1940. Sometimes he slips in passages of explicit and connected cultural and social history; this reviewer found especially interesting the pages (167–70, 177–84) on the effects of the expansion of the Dutch presence in Indonesia during the economic prosperity of the early twentieth century. These included the creation of a more European Dutch society and the decline of open concubinage with Indonesian women as more Dutchmen brought their wives to settle in the east. There are good things on experiences in the Japanese prison camps during World War II, on the Dutch-Indonesian conflict of 1945–49 (with some striking analogies to the U.S. War in Vietnam), and much more.

The author's criteria for inclusion have been largely literary. This has led to rather slight treatment of the nearly two centuries of the Dutch East India Company and to more detail than the social and cultural historian is likely to want on recent writers looking back on Indonesia or going back to visit. The discussion of E. Douwes Dekker and his famous novel *Max Havelaar* probably is adequate for a Dutch audience but assumes more familiarity with the man and his work than many readers of this translation will have. It is unfortunate that the series editor, also following literary criteria for inclusion, has omitted from the translation chapters of the Dutch original that dealt with magazines, the theater, and popular music, which would be very interesting to the cultural historian.

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R. H. W. REECE. *The Name of Brooke: The End of White Rajah Rule in Sarawak*. New York: Oxford University Press. 1982. Pp. xxxi, 331. \$49.00.

The Brooke Raj constituted a fascinating and singular Southeast Asian polity. In the past few years several scholars have penetrated below the romantic surface portrayed by earlier writers to begin critically dissecting the political, social, and economic currents of Sarawak under Brooke rule. This valuable volume by the Australian historian R. H. W. Reece discusses in meticulous detail one of the most neglected periods of Sarawak history, the two decades between 1930 and 1950, which saw the end of Brooke rule and its replacement by a British colonial administration. The author skillfully unravels the byzantine complexities and heated passions of these murky and controversial years by making excellent use of interviews and the scattered documentary record, including diaries, letters, and personal memoirs.

Reece gives us the most complete analysis available in English of the nature of late Brooke rule, the conflicts within the Brooke family and government, the critical impact of the Japanese occupation, British policies and intentions, and developments surrounding the cession to Britain in 1946. In his view, a Brooke regime traditionally characterized by personal autocracy had more or less run its course by 1941, undermined as it was by personal rivalries, succession struggles, inconsistent leadership, growing indigenous political consciousness, and a widening rift between centralizing bureaucrats and autonomy-loving outstation officers. The book contains a particularly good analysis of the rise of political awareness and competing nationalisms among the Sarawak population, as well as the catalytic roles played by the Japanese occupation and cession. The anticeSSION movement of the late 1940s emerges as a surprisingly complex and diverse phenomenon, with conservative, chauvinistic Malay aristocrats struggling for leadership against progressive, non-aristocratic Malay intellectuals seeking both multi-ethnic coalitions and social change.

Few of the many participants in the political wrangling emerge from this account with their reputations enhanced. The third rajah, Charles Vyner Brooke, comes across as capricious, greedy, vacillating, overly shy, and astonishingly disinterested in his responsibilities. His nephew, heir apparent, and chief antagonist, Anthony Brooke, is portrayed in a more sympathetic light but still with many faults: intense ambition, inflexibility, impetuosity, and a desire to perpetuate benevolent autocracy. Then there is G. T. MacBryan, who flits uneasily through the tangled web of late Brooke politics, a man characterized as complex, talented, manipulative, and unbalanced, altogether a sinister and corrupting influence. The cast also includes feuding Malay chiefs, Chinese tycoons, and Iban politicians, few of whom consistently act in a high-minded and unselfish fashion. Added to the mix are British

government officials and agents conniving for years to replace the Brookes with a regime more closely controlled by London.

The decline and fall of the Brookes is a complex saga, but Reece provides more details than are needed, clogging his narrative with considerable redundancy and unnecessarily long quotations from correspondence, diaries, and government documents. The uninitiated will not find it easy to follow the bewildering series of personalities, events, or diplomatic thrusts and counterthrusts. Nonetheless, this is a well-researched, well-documented, and altogether convincing account of an important period in Bornean, Malaysian, and British Commonwealth history.

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PIERRE ROUSSET. *Communisme et nationalisme vietnamien: Le Vietnam entre les deux guerres mondiales*. Paris: Galilée. 1978. Pp. 254.

The ability of the Communist movement to pose as a legitimate representative of Vietnamese nationalism is generally recognized as a crucial factor in its success in the recent war. The search for the historical roots of this phenomenon forms the central theme of Pierre Rousset's new book under review here. Rousset places the emergence of the Vietnamese Communist movement within the context of a broad national and social crisis provoked by the French conquest. From the outset, he points out, the young Indochinese Communist party (ICP) reflected the dual aspirations of national independence and social revolution. This was not always easy, however, for ICP policies were made by the Comintern in Moscow. As a result, the issue of national independence was subordinated to the changing needs of Soviet foreign policy. Only in the late 1930s, when the Soviet Union was preoccupied with the threat of war with Germany, was the ICP able to formulate a strategy that reflected national realities rather than the interests of Moscow.

This is a familiar theme, but Rousset presents it with a twist. Unlike some recent studies, which have portrayed the emergence of an indigenous strategy in Vietnam as a direct consequence of the coming of World War II, Rousset contends that the national character of Vietnamese communism was present from the beginning. While party leaders recognized the value of Comintern assistance and were frequently forced to yield to the needs of the international Communist movement, they persistently attempted to adapt Moscow's advice to the concrete realities of Vietnamese society and often paid only lip service to its directives. The shift to an essentially

national strategy in 1941 was thus not a sharp break from a pattern of unquestioning obedience to Moscow but the product of a gradual process with its roots in the earliest years of the movement.

This is an interesting hypothesis, which Rousset supports by citations from journal articles and party documents of the 1930s. He is certainly correct in pointing out that there was periodic grumbling over Comintern strategy within the ICP and that party leaders occasionally attempted to adapt Moscow's directives to local needs. It is less clear, however, that they were deliberately setting out to subvert Comintern instructions and to find their own road to communism. The fact is, although some ICP leaders may have sensed the need for an approach relevant to Vietnamese conditions, there is little evidence of serious debate within the party over the appropriate strategy to use until after the Sixth Plenum in late 1939. Although Rousset's general hypothesis as to the national origins of the Vietnamese Communist movement seems valid, the fact remains that it was only with the coming of World War II that party leaders felt sufficiently confident to formulate a strategy that placed the needs of the Vietnamese revolution above those of the international Communist movement. Once that process had started, of course, the road to a specifically Vietnamese form of communism had clearly begun.

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GEOFFREY SERLE. *John Monash: A Biography*. Melbourne: Melbourne University Press. 1982. Pp. xv, 600. \$42.00.

On June 1, 1918, John Monash was appointed commander-in-chief of the Australian Imperial Force on the western front in the First World War. Some Australian historians have maintained that he was the best Allied general during that war. Others have gone further and made the more extreme claim that Monash either won the war or played the major role in the winning of it.

Monash was also a man of parts. He was an accomplished pianist. He was a great lover. He was a very distinguished engineer. After the war he added to the long list of his illustrious achievements by his successful inauguration of the State Electricity Commission in Victoria. In the autumn of his life honors were showered on him: a knighthood from George V, honorary degrees, the freedom of cities. And finally a university in Melbourne was named after him.

Superficially his life reads like a story of early poverty followed by riches and fame. John Monash came to Melbourne in Victoria, Australia as a son of a German Jewish migrant. By great industry and

sheer talent he overcame the handicap of his origins; for, although Australian society was not aggressively antisemitic, in the years when Monash was climbing to the top there were heard expressions such as "Levantine," "Hebrew," or "not one of us," which reminded him that the circumstances of his birth might have excluded him from entry into the circles of the high and mighty.

Yet Monash managed to rise to the very top. When he got there, he did not find, like Old Mother Hubbard, that the cupboard was bare, but rather that he was the victim of a whispering campaign that left him possibly haunted and hunted by the fear that colleagues at the top were prepared to use his vast talents but not to accept him as one of their own. Monash was to remain the eternal outsider even when he finally got inside the halls of the men of power and renown.

There were reasons for this. Monash was not given to suffering fools gladly. He had an abrasive tongue and did not believe in trying to curb it. He was never able to claim with the Psalmist: "I held my tongue, though it was grief and agony to me." Monash was also widely suspected at the time, and indeed ever since, of flirting with the authoritarian political movements that mushroomed in Australia during the period 1919–22, when the bourgeoisie and the old country gentry were afraid of a Bolshevik revolution. It was said that Monash was a model for D. H. Lawrence's *Kangaroo*. The other reason for misgivings about Monash was his treatment of his lovers. He seems to have needed women but to have thought of them and used them as beings created to minister to his delight.

The great virtue of this most excellent life by Geoffrey Serle is that he has managed to sort out fact from myth, information from legend, and to create at least the outer man. If he has not been quite so successful with the inner man, that is because at a certain point in his life, rather like his illustrious contemporary and mighty opposite, Archbishop Daniel Mannix, Monash decided not to go on writing down his private thoughts in his diary. Rather like his cantankerous, brilliant, witty, warm-hearted rival for prominence, William Morris Hughes, he seems to have decided that no one was going to know what was going on in the heart of John Monash when he landed in Gallipoli in April 1915, when he took part in the blood bath at Pozières, when he designed the plan for an Allied victory, or when he knelt at the feet of the king emperor to receive his knighthood.

Serle has proved abundantly that there is enough in the outer man to make a very readable and informative biography. Those who want the drains to be taken up and to sniff around for some flaw or some indecency will have to wait for a more speculative biography. This book has been rightly ac-

claimed and widely praised in Australia. Perhaps Australians, because of their own savage past, fight shy of the man within. Perhaps the melancholy look in the eyes of John Monash is a painful reminder of their own inner responses to living as exiles from European civilization in a country where the "weird scribblings of Nature" caused the first Europeans who saw them to cry out in horror. Perhaps Monash shared the wisdom of his people and the wisdom of the ages when he insisted on "pulling down the blinds."

MANNING CLARK

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H. WARREN BUTTON and EUGENE F. PROVENZO, JR. *History of Education and Culture in America*. Englewood Cliffs, N.J.: Prentice-Hall. 1983. Pp. xvii, 379. \$21.95.

It is hard to know how to review a textbook. This one is workmanlike. It is more recent than Robert Church and Michael Sedlak, *Education in the United States*, the standard text, and thus covers newer work in the field. It is more balanced than David Nasaw, *Schooled to Order*. But it is not very imaginative or exciting. It is less interesting to read than Church and Sedlak or Nasaw. And it does not illuminate the issues in ways that excite historians and would-be historians. From this book it would be difficult to understand why historians feel passionately about their craft and why students ought to be aroused by the questions historians pose.

H. Warren Button and Eugene F. Provenzo, Jr., cover the standard categories. Part 1 covers "Beginnings" with chapters on "European and Colonial Sources" and "Culture and Schooling Before the Revolution." Part 2, "Age of the Common Man," discusses "Schooling in the New Republic," "The Rise of the Common Schools," "The Growth of the Common Schools," "Colonialism and Schooling," and "Schooling and Industrialism." Part 3, on "From Farm to City," has chapters on "Progressive Reform," "Efficiency and Management," and "Scientific Pedagogy." Part 4, "From Depression Until Now," concludes with "Search for Competence," "Quest for Equality," and "Pursuit of Freedom."

The interpretations are not profound. The chapters in part 3, for example, conclude that the strands of the Progressive reform movement in education included those who sought to improve the environment in which people lived and were educated, those who wished to centralize institutions in the interests of efficiency, and those who turned to science to inform pedagogy and curriculum.

There are, nonetheless, nice bits of information, for example, insights into Lewis Hine's photo stories as educator. But, not surprisingly, the material is truncated; bits and pieces rapidly spin by. There are lots of people in this textbook, but, as with most textbooks, there are few actors. Instead, events happen; the passive voice predominates. Balance turns into such profundities as "The science of education, like most human enterprises, has experienced success and failure" (p. 234). The authors, quite rightly, want to disabuse students of a blind faith in science, but they replace one form of authority with another, the authoritative pronouncements of the textbook writer, pronouncements that stand outside the vital contestations of time and place.

My preference is not to reduce history, certainly not a field like American educational history, to a single textbook. Using books like Carl Kaestle's *Pillars of the Republic*, David Tyack's *The One Best System*, and Lawrence Cremin's *American Education or Traditions of American Education* provides more than enough of a balanced overview. Most of all, I am afraid that textbooks like this make it difficult for students to be excited by history or by historians. And that is both a loss to our understanding of the past and a disservice to the historians who work with the past.

MARVIN LAZERSON
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BETTINA APTHEKER. *Woman's Legacy: Essays on Race, Sex, and Class in American History*. Amherst: University of Massachusetts Press. 1982. Pp. xii, 177. Cloth \$16.50, paper \$7.95.

A new generation of scholarship has begun the task of unearthing the history of black women in America. In the process of doing so it is also bringing new perspectives to the full range of issues that touched black women's lives. That this field of inquiry is relatively new, or that its practitioners tend to be themselves black and female or white women schooled in the civil rights struggles of the 1950s and 1960s, raises again the political realities that undergird scholarly inquiry. In a world that renders black women doubly invisible and peripheral, who will ask the questions about their history?

Bettina Aptheker's book of essays prompts such thoughts not only because of its subject but also because Aptheker herself assumes that her task in *Woman's Legacy* is simultaneously scholarly and political as are the Marxist and feminist perspectives she brings to her analysis. The principal contribution of this passionate book is its insistence on the centrality of the black woman's experience. It is a book of naming: reiterating the complex layers of racial and sexual oppression, pointing to the intersection of

struggles for racial and sexual equality, listing prominent but forgotten black female leaders. In introductions to each section Aptheker describes the personal processes that led her to each essay. As she notes in the beginning, the work is "partisan, activist, and inherently autobiographical" (p. 7). Its logic lies most clearly in Aptheker's personal life (for example her childhood friendship with W. E. B. DuBois, which is movingly described) and her concerns about contemporary action.

Many of the essays are beginning attempts, raising provocative ideas that deserve more extensive and rigorous exploration. There are logical leaps in which causality is suggested, for example when she argues that the Moynihan thesis was created to counter the growing political and economic power of black women, but the mechanisms of causality are not explored. And there are points at which she presumes a Marxist audience. On the other hand, she suggests that an analysis of the black, female experience requires movement beyond the categories of traditional Marxism, but the critique remains for the most part implicit. Greater theoretical clarity is likely to come from future work that moves beyond an analysis of oppression and of exceptional women to look at ordinary black women's lives and their relations to religion, family, and work.

Woman's Legacy is a call to action and a call to scholarship. Aptheker's honesty about her own assumptions and commitments provides a model of committed scholarship and encourages argument and debate. Future challenges to her interpretations, however, will in all likelihood support her central premise that the history of black women is a critically important lens on the American experience and its omission has left serious distortions that have consequences relevant to both the past and the present.

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EDWARD KEYNES. *Undeclared War: Twilight Zone of Constitutional Power*. University Park: Pennsylvania State University Press. 1982. Pp. ix, 236. \$17.95.

This book deals with a great issue of constitutional government—how to allocate the power to make war. Only Congress can declare war, but formally declared wars are rare. The interesting questions under our Constitution concern how it allocates the power to engage in undeclared hostilities, or to authorize military or diplomatic measures that may lead to war. As Edward Keynes's subtitle suggests, these issues are in the "Twilight Zone of Constitutional Power."

The book begins by examining the Constitution's background and provisions, insofar as they relate to

war. It then describes the role of the federal courts in reviewing executive-legislative conflicts, including the devices for avoiding review, such as the political question doctrine. Keynes explains how the scope of judicial inquiry in resolving war powers issues narrows as the issues reviewed become more international, and that the president's power is at its greatest when his acts are authorized by Congress, and at its lowest when he acts against the will of Congress. The "zone of twilight," as used in Robert H. Jackson's masterful analysis, describes those actions that are neither approved nor disapproved by Congress, where "any actual test of power is likely to depend on the imperatives of events and contemporary imponderables rather than on abstract theories of law" (*Youngstown Sheet & Tube Co. v. Sawyer*, 343 U.S. 579, 637 [1952] concurring).

Keynes's book is largely concerned with the war in Vietnam. He divides the cases on the war's legality into two groups. The earlier cases he characterizes as reflecting judicial self-restraint in "entering the twilight zone." In the cases decided after 1970 he finds that the courts "explored" the twilight zone because by then the political question doctrine had been relaxed and the Gulf of Tonkin resolution had been repealed. But in reaching the merits, the courts largely upheld the executive, leading Keynes to conclude: "Unless Congress exercises it[s] constitutional authority to restrain executive power, opponents of constitutional dictatorship should not await a judicial David to slay an executive Goliath." (p. 160).

The book is well written and interesting. The summary of the litigation sparked by the war in Vietnam is valuable. But the book is not an authoritative analysis of the role of the Constitution or of the federal courts in controlling undeclared war. Keynes concludes, for example, that the Constitution allows the president to take "defensive" but not "offensive" actions because it grants Congress the power to declare war. Yet, while some of the framers wished to limit executive authority to "defensive" actions, nothing in the Constitution or in its early application approved this distinction, which has proved untenable in practice. The same distinction led Keynes to redefine Jackson's twilight zone to mean those executive actions that could be characterized as either "defensive" or "offensive." But Jackson's twilight zone includes those executive actions, whether characterized as defensive or offensive, that are neither clearly authorized nor clearly prohibited by Congress. The author's redefinition enables him to see the Vietnam War in the twilight zone, although it was clearly authorized until Congress removed its approval.

ABRAHAM D. SOFAER
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S. CHARLES BOLTON. *Southern Anglicanism: The Church of England in Colonial South Carolina*. (Contributions to the Study of Religion, number 5.) Westport, Conn.: Greenwood Press. 1982. Pp. xiv, 220. \$29.95.

S. Charles Bolton has provided the balanced, scholarly study of South Carolina Anglicanism that colonial scholars long have needed. Based on extensive research in primary sources, and using the revisionist studies of other scholars of colonial Anglicanism, this book provides a clear institutional study of the Anglican establishment and its ministers.

Bolton argues that internal divisions and external pressures shaped the South Carolina establishment into a strong church with competent ministers and extensive lay control. Establishment of the Anglican church in 1704 was an outgrowth of English missionary pressures and political-religious division in South Carolina. A coalition of low-church Anglicans and Huguenots pushed through an establishment bill after excluding dissenters from the Carolina house. Because the bill created a lay commission to oversee parish elections of ministers and remove erring clergy, high-church Anglicans joined dissenters in opposing the bill. The assembly revoked the commission's removal power in 1706 to forestall royal disallowance of the 1704 act. When Gideon Johnston, Carolina's first commissary, arrived two years later, he tried to provide church leadership. The early commissaries slowly achieved control over erring or factious clergy, although Johnston had continuing trouble with Huguenot clergy who refused to conform, and his successor, Alexander Garden, had showdowns over pluralism and George Whitefield. Garden's tireless work to gain lay support for the church resulted in a decision by the SPG by 1740 that South Carolina no longer needed their support.

After mid-century, lay control over the church increased as it became a colonial institution. The SPG no longer recruited and disciplined missionaries in South Carolina. The power of the bishop of London and the commissaries waned. Clergy conventions replaced visitation by the commissary. Parishes recruited their own clergy and kept them on annual contracts in order to dismiss scandalous ones. Bolton reverses the usual wisdom by demonstrating that parish control over hiring and firing of clergy led to an improved ministry.

Disestablishment during the Revolution came not as a reaction to a weak, disreputable church, but as the natural outgrowth of lay control and the failure of the Anglican church to reach the frontier and lower classes. Bolton argues that, while people of all classes attended Anglican parishes, the church became the bulwark of gentry beliefs. Newer settlers thus were not drawn into the Anglican fold. Thus

when lowland patriot leaders needed to woo the dissenter backcountry to support the Revolution, they replaced an Anglican establishment with a general Protestant establishment. Anglicans in South Carolina, unlike those in Virginia, kept their church property, and denominations were incorporated.

Bolton has not only offered a revisionist version of South Carolina Anglicanism but has compared it to the church in other Southern colonies. As a result, the study provides a good introduction to the recent reassessment of Southern Anglicanism and has much to offer a wider audience. While more comparisons could have been made (the parallels with Virginia are much more extensive than he notes), such analysis would be the topic for another study.

JOAN R. GUNDERSEN
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J. M. SOSIN. *English America and the Revolution of 1688: Royal Administration and the Structure of Provincial Government*. Lincoln: University of Nebraska Press. 1982. Pp. 321. \$25.00.

This is the second volume of a projected three-volume history of England's American empire, 1660–1714. The first, *English America and the Restoration Monarchy of Charles II*, was published in 1980, and a review by Richard Dunn appears in this journal for October 1982. Here J. M. Sosin picks up the story in 1685 and carries it to about 1700. The build-up of colonial warfare with France and the Indian relations that went with it will presumably be treated in the third and final volume.

One cannot read far into Sosin's book without a sense of *déjà vu*, for his history is like nothing so much as the old imperial school. The tone is consistently favorable to England, the narrative element is strong, and the use of English archival sources is awe inspiring. When completed, Sosin's three volumes promise to be the most comprehensive study of England's imperial policy during the period and a worthy revision of C. M. Andrews's *England's Commercial and Colonial Policy* (1938).

It is all the more unfortunate, therefore, that Sosin has defined his project so narrowly as to exclude the English West-Indian sugar islands. To write about the structure of England's colonial empire without reference to the sugar colonies makes little sense. I had thought that after Richard Dunn's *Sugar and Slaves* and Carl Bridenbaugh's *No Peace Beyond the Line*, both published in 1972, that parochialism was past.

By coincidence the connection between the islands and the mainland is one of the most interesting themes of Stephen S. Webb's *The Governors-*

General, The Royal Army and the Definition of Empire, 1569–1681 (1979). This work, which unfortunately seems to have appeared too late for Sosin to take it into account, is sure to be the interpretation with which Sosin's will most often be compared. Sosin implicitly rejects, at least for the years after 1689, Webb's thesis that the army and prerogative played a major role in colonial administration. One hopes that he will come to grips with this interesting contrast of views in his next volume.

The main themes of Sosin's history are the intention of the crown to bring the colonies to a greater dependence on England and the frustration of this policy, in part by the political instability of the colonies. Explaining that instability, Sosin writes, "there were more men who aspired to higher political office . . . than could be accommodated within the existing governmental arrangements" (p. 29). For some the crown policy was a threat to their continuation in power; to others it "created an avenue to honor, recognition, and profit" (p. 30).

Sosin is skeptical of professions of principle by colonial trouble makers. Arguments based on Magna Carta and the rights of Englishmen, he writes, "often indicated that they needed to appear virtuous even while pursuing their own interests" (p. 30). This skepticism of ideology as a spring to action makes a sharp contrast to the one other full-dress study of the 1689 rebellions, David S. Lovejoy's *The Glorious Revolution in America* (1972).

These theoretical positions about how and why people chose up sides cause some difficulty, at least in regard to Massachusetts. For example, Joseph Dudley's willingness to cooperate with the crown can hardly be explained, as might be done for Jacob Leisler or John Coode, by Dudley's not having access to political power. Or again, Sosin repeatedly refers to Samuel Nowell and Increase Mather as "commonwealthmen," as if whatever republicanism they had came more from Magna Carta than from belief in the New Jerusalem.

Nevertheless, I find Sosin's narrative on the whole first-rate and his interpretation of events stimulating. His account of Increase Mather's activities in England, 1688–91, for which he has identified key documents in the Public Record Office, is excellent (pp. 137–38). So much of the book disparages Puritan resistance to Sir Edmund Andros and the Dominion government as mere self-service that one might consider this a kind of Tory history, setting the record straight from the English point of view.

The narratives of rebellions in New York and Maryland seem carefully done, although occasionally Sosin has missed good work, such as Robert C. Ritchie's *The Duke's Province* (1977). Leisler and Milborne are presented as "frustrated and resentful opportunists." The story in Maryland follows closely the conclusions of Lois Green Carr and David

Jordan in *Maryland's Revolution of Government, 1689–1692* (1974) but is grounded on meticulous research in the documents.

Sosin provides vignettes of each colony and many quick and always interesting profiles of people: William Penn (a weak proprietor), Sir Henry Ashurst (of poor and often irrational judgment), John Usher, Bellomont, and many others. He deals only slightly with the Navigation Act of 1696 but at length with the Board of Trade.

Sosin makes his most important contribution by giving meaning to the rebellions of 1689. "Boston in the spring of 1689," he writes, "was the key to the momentous events which determined the political configuration of English America for almost one hundred years" (p. 80). The rebellions in Boston, then New York and Maryland, brought to a halt attempts to govern the colonies by prerogative. Similarly, the revolution in England persuaded Whitehall to seek change through Parliament rather than by prerogative (p. 156). In the end, however, Parliament proved uninterested and imperial administration continued to operate by "personal connection and private interest." These conclusions will be controverted, but Sosin has made a substantial case for his interpretation and an important addition to the literature about the period.

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ROBERT W. TUCKER and DAVID C. HENDRICKSON. *The Fall of the First British Empire: Origins of the War of American Independence*. Baltimore: Johns Hopkins University Press. 1982. Pp. viii, 450. \$24.00.

At the end of the Seven Years' War in 1763 when "the sun never set on the British flag," the British empire was not only at its apogee but also faced unknowingly a partial eclipse of its own bright star. The far-flung domain, achieved by somewhat the same martial means as the Roman empire, now stood at a vital juncture, and the wrong judgment could lead to a similar demise.

The American colonies had mainly supported the British in their conflict with the French and Indians and were thus assumed by England's leaders to be a lasting part of the empire. But of course such was not the case. Now two political scientists of Johns Hopkins University, Robert W. Tucker and David C. Hendrickson, in their new book have challenged the views of conventional historians in regard to the causes of America's War for Independence.

Imperial historians of the period, led in eminent logic by Lawrence P. Gipson, who is sometimes overlooked in recent analyses, have contended that the American Revolution took place in the context

of an extensive and successful colonial administration. Yet just twenty years after the Seven Years' War, Britain formally lost its American colonies; and this year, in this country and in England, the bicentennial anniversary of the Treaty of Paris is being mildly commemorated. (This should have been a climax to a continuing United States Bicentennial celebration of the American Revolution.)

Tucker and Hendrickson raise provocative questions. Was there a well-defined status quo in the imperial-colonial relationship in 1763? Was Parliament justified constitutionally in insisting on its government of the Americans? Was Lord North stupid in his handling of the colonial crisis, or was he hopelessly handicapped by King George III and Parliament? (The more favorable treatment of Lord North is one of the better parts of this book.) These and other incisive questions are posed, but unfortunately the authors do not always furnish satisfactory answers. True, they cite endless sources, add footnotes that are sometimes longer than their pages of text, but at book's end, one does not derive a clear impression of their general conclusions.

From the Grenville reforms through the Stamp Act crisis to the decision for war in 1775, the authors attack the inconsistent imperial attitude and contend that the behavior of Britain was due to the need to preserve effective control over the American settlements and still avoid armed coercion. The First Continental Congress in 1774 requested, "Place us in the same condition that we were at the close of the last war, and our former harmony will be restored." Regarding this, the authors convincingly show that no matter what Britain did, the Americans were still determined to free themselves of the English yoke. Ironically, the British at this time considered their empire in North America to be the principal source of their wealth and power. And if, as J. H. Plumb has stated, England was then ruled by thirty wealthy men, one can readily grasp why they were reluctant to lose their chief foreign asset.

Physically, the book is a handsome one. The writing, however, is marred at times by labyrinthine sentences that wind away into a wilderness of uncertain meaning. Minor items: the use of the word "principle" when "principal" is meant; the misspelling of the David Ramsay as "Ramsey"; and the use of the hackneyed expression, "early on," and the over-worked verb, "address." But on the whole, the book is a valuable contribution to the literature of the American Revolution and the first British empire. With the present decline of what was once Great Britain it evokes nostalgia for the times long before Kipling announced, "the captains and the kings depart."

NORTH CALLAHAN
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ROBERT MIDDLEKAUFF. *The Glorious Cause: The American Revolution, 1763–1789*. (Oxford History of the United States, number 2.) New York: Oxford University Press. 1982. Pp. xvi, 696. \$25.00.

Robert Middlekauff's new narrative history of the American Revolution is the first volume to be published in C. Vann Woodward's projected eleven-volume *Oxford History of the United States*. At least twenty years in the making, the series has two goals: first, to offer "a readable text that will be readily accessible to the educated general public," and, second, to "provide students at various levels an interpretative synthesis of the findings of recent scholarship as well as the essentials of narrative history in the period or subject being treated" (p. xvi). Middlekauff's book amply fulfills the first of these objectives. The style is lively, the narrative flow rapid, and the scholarly analysis unobtrusive. With regard to the second objective, most specialists will probably regard the volume as a curiously unbalanced and somewhat idiosyncratic work that, far from being a comprehensive synthesis of recent scholarship, largely ignores several of the more important interpretive problems raised by that scholarship.

At least since the publication of the *American Nation Series* early in this century, specialists have conventionally divided the Revolutionary era into three roughly equal segments: the preliminaries between 1763 and 1775, the war and the political reorganization of the states between 1775 and 1783, and the Confederation period between 1783 and 1789. Although Middlekauff does not explicitly challenge this conventional breakdown, he devotes almost 50 percent of his text to the war years with the first period taking up slightly under 40 percent and the third just over 10 percent, a distribution that reveals Middlekauff's conviction that the military events that led to the establishment of American independence were the most significant aspects of the Revolutionary era.

Relatively few modern students of the Revolution share this conviction; nevertheless, it forms the basis for Middlekauff's most striking achievement. As the volume's centerpiece, his section on the war years stands with Don Higginbotham's *The War of American Independence* as one of the two best recent narratives of the strategies, campaigns, and battles of the Revolution. Informed by an acute awareness of how the character of American society affected the performance of the American army and how the difficulties of subduing a large, rebellious population perplexed British military leaders, this section is filled with penetrating assessments of the principal actors on both sides, and on the one occasion when Middlekauff departs from a narrative structure, he provides an unusually perceptive analysis of

why ordinary citizens found the American cause sufficiently "glorious" to fight in it. Notwithstanding these many virtues, the section on the war years will disappoint many scholars because of its relatively cursory treatment of the diplomatic and domestic aspects of the war, which together take up only about one-tenth of the pages devoted to the war. In particular, the awkward plight of the "loyalists"; the widespread popular resistance to, or at least disinterest in, the war, especially during its later stages; and the impact of the war on the societies and politics of the states are subjects that, while not totally neglected, by no means receive attention commensurate with recent scholarly interest in them.

Only slightly less ample, Middlekauff's treatment of the pre-Revolutionary years provides an equally crisp and intelligent recounting of the unfolding controversy between Britain and the colonies. His interpretation of this controversy as primarily a constitutional and ideological one in which Americans at once both sincerely believed what they were saying and were animated by a culturally derived, "almost paranoid" (p. 128) fear of "a conspiracy that had no basis in fact" (p. 131) closely follows the neo-Whig interpretation worked out in the 1950s and early 1960s by Edmund S. Morgan, Bernard Bailyn, and others, albeit, in an original and intriguing suggestion that bears further consideration and refinement. He emphasizes the extent to which colonial behavior was conditioned as much by the colonists' seventeenth-century religious heritage as by the radical political tradition stressed by Bailyn. Yet, this section, like the one on the war, does not systematically and fully confront a number of important problems that have interested recent students of the origins of the Revolution. These include why the British government initially undertook its restrictive measures against the colonies; whether, given the obviously restrictive intent of these measures, it makes much sense to describe the colonial response to them as in any sense paranoid; and what bearing social and political tensions in the colonies had on that response.

Because it is so slight, the final section on the Confederation and Constitution, which also includes a brief discussion of the earlier process of state constitution making during the war, is by far the least satisfactory as a synthesis of recent scholarship. Although it provides a brief and accurate narrative of the movement toward federal union, it is disappointingly thin in its discussion of either the complex debate, analyzed by Gordon Wood and others, over the shape and character of that union or of the political, social, economic, and ideological divisions that underlay that debate.

Middlekauff's relative neglect of so many topics that have interested students of the Revolution over

the past quarter century—and the list might be expanded to include a variety of subjects in social, economic, and cultural history—strongly suggests a deliberate decision not to attempt the sort of comprehensive synthesis prescribed by the series. Given the rich and often conflicting strands in the existing literature, this was probably a wise decision. What he offers us instead is his own personal reading of the Revolutionary experience. Because it is so engagingly presented and so obviously the product of a judicious and subtle historical intelligence, this reading will both appeal to nonspecialists and repay the close attention of scholars in the field.

JACK P. GREENE
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RONALD HOFFMAN and PETER J. ALBERT, editors. *Sovereign States in an Age of Uncertainty*. (Perspectives on the American Revolution.) Charlottesville: University Press of Virginia, for the United States Capitol Historical Society. 1981. Pp. xiv, 261. \$20.00.

Nearly sixty years ago, Allan Nevins asserted in his pathbreaking *The American States during and after the Revolution* that historians had largely neglected state developments, leaving important areas of Revolutionary history unexplored. Ever intrigued by the study of causation, historians have notably devoted much less attention to the impact of the Revolutionary struggle on the people at large. Because of the stimulating work of Merrill Jensen and Jackson T. Main, the imbalance has been gradually rectified. *Sovereign States*, a collection of eight essays, reflects graphically the continuing interest of numerous contemporary historians in local and state activities during the war and Confederation. The essays constitute the second volume in *Perspectives on the American Revolution*, ably and judiciously edited by Ronald Hoffman and Peter J. Albert. Originally presented at a symposium sponsored by the United States Capitol Historical Society and the United States Congress, they bring together new viewpoints derived from recent research on the functioning of state governments.

The essays *in toto* are well researched, informative, challenging in their interpretations, and reflect a return, in large measure, to a political analysis of the period. With this focus, most of the essays merely touch on social or economic developments at the local level. Jackson T. Main's introductory article argues cogently that the aspirations and accomplishments of the common man—the heart of the Revolution—can best be understood by concentrating on the states, not on the national scene. Stephen E. Patterson's study of the wartime emergence of Massachusetts Federalists is provocative. In his view

dynamic, property-conscious leaders, rejecting the older concepts of community consensus, moved resolutely toward political tolerance, individualism, and modernization. South Carolina, in contrast, as Jerome Nadelhaft develops his inquiry, experienced the dynamism of the back-country populace gradually wresting political control from the previously dominant Charleston elite, a triumph signaled by democratic gains in the constitution of 1790. Edward Papenfuse sees, instead, the continuing linkage of economic and political forces as a major determinant in Maryland. Guiding the reader through the maze of the state's wartime fiscal woes, he concludes that contentious factionalism precluded a satisfactory solution to the problems at the state level.

To this reviewer, Richard A. Ryerson's reinterpretation of the Pennsylvania constitutionalists, their outdated concepts of the body politic, and the failure of the constitution of 1776 is outstanding with its penetrating insights. The connection between the use of power and Revolutionary politics is the theme of Edward Countryman's detailed and valuable analysis of the New York Assembly, while Emory Evans presents the incessant difficulties of Virginia's three war governors as they operated under severe political restrictions as well as British incursions into the state. Appropriately, the concluding essay is by the late Merrill Jensen to whom the volume is dedicated.

This is a book that should prove stimulating to the specialist and extremely useful to graduate students or even advanced undergraduates. The editors have brought together an excellent collection that will reward the careful reader with a new understanding of the complexities and diversities of human interactions as the sovereign states grappled with the monumental problem of carrying out their Revolution.

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MANFRED JONAS and ROBERT V. WELLS, editors. *New Opportunities in a New Nation: The Development of New York after the Revolution*. Schenectady: Union College Press; distributed by Syracuse University Press. 1982. Pp. xi, 146.

Union College and Patroni Scholastici, a private foundation, sponsored a conference in October 1981 on the development of New York State in the period after the Revolution when it outstripped all others in almost every category of growth. Manfred Jonas edited the proceedings in capable fashion placing the footnotes in the proper place and including a score of useful maps, charts, and repro-

ductions of cartoons and engravings. Robert V. Wells provided a graceful introduction.

The opening paper by Mary Jo Kline in sprightly fashion sets the stage by stressing the "newness" of the state west of the Hudson Valley. If earlier observers underlined the geographical advantages of the Mohawk gateway, Kline has emphasized—perhaps unduly—the obstructions to westward expansion. Like most historians she has difficulty penetrating the thickets of New York politics. The findings of Philip White, Kathleen S. Kutolowski, and Lee Benson might have added more depth to her account of politics after 1800.

William Siles's article on pioneering in the Genesee country endorsed the theme of Richard Wade's thesis of urban outposts in the wilderness. Assigning the "primary" role to town planners and builders, Siles outlined the process whereby Massachusetts's claims to western New York passed into the hands of land proprietors such as Oliver Phelps and Nathaniel Gorham. Almost overnight Canandaigua emerged as a commercial center performing sophisticated economic services and reproducing the social amenities of New England.

The conference included a study of regional factors by Donald Meinig, a geographer whose analyses of Western regions have received wide acclaim. Meinig surveyed the attempts by historians and geographers to deal with New York as part of extended New England and as part of the Middle Atlantic region. Fully aware of the Yankee impact in all its complexity, Meinig also noted influences such as Methodism as well as coal moving north from Pennsylvania. Long before the coinage of the term megalopolis, postal services and travel routes were creating a New York–Philadelphia axis. Nevertheless, architectural styles, language patterns, and population stock continued to perpetuate differences between these sister states.

Michael Kammen reacted to the papers but also added his own observations in witty style. He discussed the violent oscillations between 1756 and 1830 in the population and economy of the metropolis, the section of the state largely ignored by the other participants. He also noted the optimistic and energetic attitude of state residents, partly the result of selective migration, the crossbreeding of Yankees and Yorkers, and the relative freedom from the restraints hampering New Englanders and Virginians. Neither the loss of the national or state capital checked New York's rapid expansion.

I caught only a half-dozen minor errors. Charles Grandison Finney did not grow up in Utica as stated on page 141. The conference and the proceedings have enlarged our knowledge and understanding of this crucial period in New York history.

DAVID MALDWIN ELLIS
Hamilton College

MILTON LOMASK. *Aaron Burr: The Conspiracy and Years of Exile, 1805–1836*. New York: Farrar, Straus, Giroux. 1982. Pp. xviii, 475. \$22.50.

Aaron Burr, the grandson of Jonathan Edwards and the son of Jonathan Edwards the younger, president of Princeton University, seemed from youth almost preordained for greatness. He was brilliant, suave, and enough of a swashbuckler to cut a dashing figure as a young officer during the early campaigns of the Revolutionary war. In the years that followed he became a leader at the New York bar and prominent in state politics. In 1799 Burr masterminded his most brilliant political coup when he succeeded in winning New York for the Democratic-Republican party, thus assuring a Jeffersonian victory in the presidential election of 1800.

Yet, his early triumphs notwithstanding, as Milton Lomask makes clear in his fine biography, there was something curiously alien about this descendant of the great Edwards. Young men frequently rebel against the values of an older generation. But Burr turned against an entire culture, quite deliberately modeling himself after Philip Dormer Stanhope, the fourth earl of Chesterfield. Like that noble lord he devoted his entire life to the fine art of being a gentleman.

Sophisticated, witty, and something of a roué, Burr thought the leaders of America's Revolutionary generation a boring, unimaginative lot. They took themselves and what they were about so seriously. Undoubtedly he had a point. Adams, Hamilton, Jefferson, and the others were not exactly a fun-loving group. And humor, especially of the self-deprecating sort, was definitely at a premium among our Revolutionary forebears. But Burr carried things rather too far in the other direction. He played at politics for fun and profit and seems to have believed in nothing except the game itself. Moreover, save for his cherished daughter Theodosia and her son "Gamp," he felt no responsibility toward anybody, least of all those who trusted and showed confidence in him. He squandered a fortune loaned to him by that naive romantic Harman Blennerhasset, and seems never to have thought twice about it. The illustrious Mme. Jumel, whom he married in 1833, left him almost immediately when it became clear that he was about to do for her what he had earlier done for Blennerhasset. He borrowed frequently and unashamedly from anyone willing to lend, always lived beyond his means, and never seems to have considered repaying debts if it meant that he would suffer the slightest inconvenience. Burr lived in a world of sometimes extravagant fantasies. These, however, seldom came to anything, for, while he was ready enough to seize whatever fortune might throw his way, he was not particularly adept at making things happen. He

would have taken the presidency from Jefferson had the Federalists been willing to hand it over in 1800 when they had the chance. But true to his Chesterfieldian code of nonchalance, he lifted not a finger to attain it. After leaving the vice presidency in 1804 he spent the better part of a decade pursuing his most extravagant fantasy, the conquest of Mexico and his enthronement as its emperor. But this vision, though long entertained, never approached maturity. Only romantics like Blennerhasset, or guilt-edged scoundrels such as James Wilkinson, showed much interest in his schemes. More practical men, one supposes, recognized Burr for a dreamer, not an organizer. Like a much better known exponent of the theatrical arts, Burr too thought all the world a stage and never tired of posing. But it takes more than charm and wit to achieve great things, and one gets the impression that Burr could do little more than pose.

GERARD H. CLARFIELD
University of Missouri,
Columbia

WILLIAM E. NELSON. *The Roots of American Bureaucracy, 1830–1900*. Cambridge: Harvard University Press. 1982. Pp. viii, 208. \$22.50.

The Roots of American Bureaucracy, like Stephen Skowronek's *Building a New American State*, is part of a new (or, more accurately, the revival of an old) interest in the evolution of American public institutions. Its author, William E. Nelson, a law-school legal historian, brings the special perspective of his field to this study. But he does so in a way that is of interest and importance to all American historians.

Nelson's thesis is that the public life of this country has been dominated by the clash between two ideals: majority self-rule and the protection of individual and minority rights. The first of these ideals underlay the rise of democratic politics in the early and mid-nineteenth century. Legislatures, increasingly responsive to popular sentiment, the presidency, from Jackson on, and the instrumentalist mode of judicial reasoning (most notably in Taney's *Charles River Bridge* and *Dred Scott* opinions) added up to a style of government that favored majority values over the inalienable rights—property or human—of individuals and minorities.

Slavery was the issue on which this majority wave broke. The antislavery movement offered the first powerful, reasoned opposition to majority democracy, an opposition that drew on evangelical morality, transcendental individualism, and the human rights tradition embodied in the Declaration of Independence.

After the Civil War, reformers steeped in this public philosophy of moralistic, antimajority individualism sought to apply it to some of the problems

of industrial America: political corruption and inefficiency and legislative threats to property rights. They sought new rationales for checking the majority impulse: science (evolutionary or social) and the American past, in which (they supposed) the "best men" had provided wise leadership, and political morality once had a firm communitarian base.

It was this restated ideal of individual-minority rights, Nelson holds, that nourished the roots of modern American bureaucracy. Congress came to be dominated by specialized committees, in which seniority and expertise flourished. The civil service became more professional, and with the ICC the independent regulatory commission made its appearance. The judiciary adopted a formalistic style of reasoning that was perhaps the most consequential expression of the new emphasis on apolitical, professional, nonmajority standards.

Nelson's thesis is not airtight. He too closely identifies the defense of pluralism and individual rights with the antimajority (and in the twentieth century the bureaucratic) impulse in American public life. Surely it can be argued that at least in this century majority politics has come to accept—indeed, to rest on—a pluralist base. And surely the bureaucratic temper has not always been at the service of the pluralist ideal or of minority interests.

Nor is the line of descent from late nineteenth-century genteel reform to the twentieth-century administrative-welfare state all that clear, or all that divorced from majority politics. When Nelson concedes, "Of course, political parties did not lose all their significance in the late nineteenth century Congress" (p. 118), he is in danger of distorting the true character of late nineteenth-century American public life in his desire to find in it the roots of the twentieth-century bureaucratic state.

But, in sum, he has given us a stimulating and provocative interpretation of the nineteenth-century American state.

MORTON KELLER
Brandeis University

JOHN J. DUFFY and H. NICHOLAS MULLER III. *An Anxious Democracy: Aspects of the 1830s*. (Contributions in American Studies, number 58.) Westport, Conn.: Greenwood Press. 1982. Pp. xii, 172. \$27.50.

In the 1970s John J. Duffy and H. Nicholas Muller coauthored two articles about Vermont, one on popular responses to the *patriotes* uprising in Lower Canada that took place in 1837, the other on Jedidiah Burchard and the "New Measure" religious revival of 1835–36. *An Anxious Democracy* is the product of their continued interest in Vermont's cultural history during the Jacksonian period. The monograph includes reworked versions of the earlier articles, an analysis of the educational and politi-

cal ideas of James Marsh (who as president of the University of Vermont spearheaded opposition to aiding the *patriotes*), a chapter summarizing the thought of several Vermonters prominent in the state's intellectual life, essays linking Vermont to the literary and cultural history of the outside world, and discussions of other matters loosely subsumed under the subtitle "Aspects of the 1830s." The central theme of the volume is that Vermonters, like other Americans, were disoriented by a general weakening in the traditional social institutions of family, church, and community and were therefore susceptible to movements that "promised a regenerated spirit, a reconstructed social reality, a unity of self and other" (p. 144). What tied the various "aspects" of Vermont's cultural life together was their common root in a pervasive "anxiety" about both the present and the future.

Students of the history of Vermont should find much to interest them in the book. Duffy and Muller probe more deeply into the phenomena they discuss than did David Ludlum, whose *Social Ferment in Vermont, 1791–1850*, provides the only previous systematic treatment of reform movements in the 1830s. The authors are especially perceptive in their treatment of Marsh and in the way they link the publication in 1839 of Daniel Thompson's legend-making *The Green Mountain Boys* to the rituals of *patriotes* advocacy. The details of religious revivals and fanciful political movements always make for good reading.

The volume, however, is better in parts than as a whole. Duffy and Muller engage in a lot of heavy-handed methodological discussion about the importance of local history and the psychology of social movements. Indeed, at times they seem almost as anxious about the legitimacy of their activities as the people about whom they write. Furthermore, the volume does not hang together as a piece of literature. The chapter titles—"An Aesculapius of the Soul" and "Phaedon or Bowditch" for example—confuse rather than guide. Although the authors try hard to define relationships among the various dimensions of Vermont culture in the 1830s, they have difficulty. The problem is a simple one: the subjects covered in the volume really do not fit together tightly. "Aspects" is a pretty weak rubric under which to organize anything.

Loose or tight, *An Anxious Democracy* is a useful addition to the rapidly increasing literature about northern New England. Regional historians should become familiar with its contents.

JERE R. DANIELL
Dartmouth College

WILLIAM PRESTON VAUGHN. *The Anti-Masonic Party in the United States, 1826–1843*. Lexington: University Press of Kentucky. 1983. Pp. x, 244. \$16.00.

STEPHEN E. MAIZLISH and JOHN J. KUSHMA, editors. *Essays on American Antebellum Politics, 1840–1860*. Introduction by THOMAS J. PRESSLY. (Walter Prescott Webb Memorial Lectures, number 16.) College Station: Texas A&M University Press, for University of Texas, Arlington. 1982. Pp. 229. \$19.50.

Several decades ago historians seemed to churn out studies of "Jacksonian Democracy" and the "causes of the Civil War," but of late such projects have been put on hold. History is coming at us in more finite slices. The two books under review illustrate the weaknesses and strengths of doing history in "minutely organized Particulars."

One must begin with the weaknesses illustrated by *The Anti-Masonic Party in the United States*. It is a narrow book on a narrow but important topic. At the turn of the century several dissertations were written on the Anti-Masons, but interest in the topic petered out. There are two brief and unflattering sentences referring to the movement in *The Age of Jackson*. Lee Benson's focus on the importance of the New York Anti-Masons led to revival of interest in the subject illustrated by the excellent work of Dorothy Lipson, Kathleen Kutolowski, and Ronald Formisano among others. Most of the recent studies have tended toward an Anti-Masonic view. Thus, this book by a Mason and sponsored by Masonic money might easily be looked on as a response to recent scholarship.

Unfortunately this book defines the topic in a limited fashion and is hardly a synthesis of the latest scholarship on the subject. William Preston Vaughn is not, however, a simple apologist for the order. At the beginning of the book he states—and later repeats—that he agrees with "Victory Birdseye that Morgan was probably murdered by misguided Masons," and agrees that there was a cover-up. Yet, he defends the order and focuses primarily on the effect of Anti-Masonry on "the fraternity."

There is no overall thesis to the book that is generally descriptive. Chapters deal with the origins of the movement, its growth in several states, and its role in the presidential elections from 1832 to 1840. Yet, Vaughn fails to address the major questions posed by Anti-Masonry. Although he is interested in the relationship between Masons and Anti-Masons, he never closely examines the social bases and functions of either. He also refuses to address systematically the role of Anti-Masonry in the emergence of the second party system. Consequently, this book reflects none of the sophistication present in the recent published and unpublished literature on the subject. *The Anti-Masonic Party in the United States* is a retelling of an old tale that adds little to what historians know about Anti-Masonry.

In contrast, *Essays on American Antebellum Politics*, edited by Stephen E. Maizlish and John J. Kushma

is an exceptional book. Such collections are usually uneven, unbalanced, and horribly difficult to review. This book, however, opens up the history of antebellum politics in ways that few monographs have done. Each of the essays is longer and better than the average ones published in the major journals.

All of the authors agree that political parties were of supreme importance during these years and that they were deeply rooted in the social, economic, and psychological soil of the time. Each plays off his argument against this basic reality. William E. Gienapp sets the tone with his superb essay on the political culture of the North. Its title, "Politics Seem to Enter into Everything," states his thesis, which is developed with a command of the sources and a balanced intelligence that is striking. Thomas B. Alexander does what he has always done well in his essay "Voter Partisan Constancy in Presidential Elections." He shows that historians can only understand revolutionary change by recognizing certain elements of continuity. The political realignment of the 1850s must be understood in relation to what political scientists have termed "the normal vote." Alexander's emphasis on continuity forces historians to face certain facts that do not fit well with most interpretations of the causes of the Civil War. He may be faulted for continuing to talk about "party unity" when institutions ceased to exist and when electoral groups are unified only by his adding them together, but it seems impossible after his work that any right-minded historian could write about each of these elections as a totally individual event. Michael F. Holt's essay on the problems of the Whigs in the mid-1840s focuses on the detail of political maneuvering. Essentially Holt tries to answer the question: Why did the Whigs turn to Taylor in 1848? His answer is that the Whigs acted like good politicians must and responded to a less than hospitable environment. By showing how Whig leaders weighed various alternatives in the face of changing circumstances, Holt provides the best and most reasonable analysis of this process available. Maizlish's essay handles a difficult problem that most Civil War historians have muffed badly. It is probably the best short statement of "The Meaning of Nativism and the Crisis of the Union." The author, however, tries too hard to give a "middle-of-the-road" interpretation between the ethnocultural and traditional views and pays too little attention to just who became a nativist. The final essay by Joel Silbey is an excellent examination of the old question: Why did Southern Democrats respond in such an extreme fashion to the election of Lincoln? He has shown before that both Northern and Southern Democrats shared a common set of ideas about their opposition. Using this, Silbey attempts to bring together the findings of those who have studied

secession and those who have focused on the political realignment of the 1850s. His conclusion is convincing, but the reader is left asking: Why in this highly partisan atmosphere did so many Southern Whigs and Northern Democrats each follow their section in the bloodletting? Aside from five excellent essays there is a somewhat cantankerous introduction from Thomas J. Pressly. What is important about this book is that much of the power of the essays by Holt, Maizlish, and Silbey comes from an understanding of the environment of antebellum politics established by Gienapp and Alexander. The chemistry among the authors gives the total a marvelous texture. This is a book that should be in paperback for use in upper-level classes.

Books such as these show that the political history of antebellum America is alive and well. New methods and new blood are making it more interesting than ever, as the questions political historians ask reach out in ever-widening circles.

WILLIAM G. SHADE
Lehigh University

ROBERT C. FULLER, *Mesmerism and the American Cure of Souls*. Philadelphia: University of Pennsylvania Press. 1982. Pp. xvi, 227. \$20.00.

Robert C. Fuller, who teaches religious studies at Bradley University, has used a history of mesmerism in America as a vehicle to attack religious groups and also parareligious groups such as transcendental meditation for their narcissistic preoccupations. The history of the cure of souls, Fuller maintains, confirms theology; without ethics and brotherhood, religious movements cannot reach the divine, even through the deepest introspection.

Before the Civil War, most American students of mesmerism were healers or showmen or both. In the course of healing, they discovered a hierarchy of types of communications with a natural force at first understood to be animal magnetism. Later they came to believe that the effectiveness of mesmerism was a function of the extent to which an individual's psychology could open itself to new and sublime levels of reality. With the popular work of Phineas P. Quimby, this acceptance of the validity of subjective data functioned in the culture to help explain to Americans how to find certainty and purpose in a rapidly changing, increasingly secular world. By making thought the cause and cure of human ills, Quimby and his followers (not least of whom was the founder of Christian Science) felt that they could regain control of their lives.

Quimby expanded mesmerism into a general "philosophy of life," but in the late nineteenth century "New Thought" diluted it, Fuller says, into advice to follow inner promptings and to approach

the world in terms of subjective wants and projected willfulness. What had been inner sanctification became now discovering the "true" inner self. Such popular interpretations, Fuller points out, appealed especially to "unchurched" Americans.

Most of the previous work dealing with American mesmerism has tended to be technical history, and Fuller's account of the popular literature is therefore a welcome addition. He comments on the relative naturalism of mesmerism as opposed to spiritualism, and he notes that phrenology did not possess the semireligious appeal that mesmerism did. But otherwise Fuller does not fully use—and seldom cites—a substantial body of secondary works in history and American literature that would have added greatly to his account. Only three-quarters of the way through, for example, does he take any note of the work of Gail Thain Parker and Donald Meyer, and this reviewer could not find Richard Weiss's history of "New Thought" in either text or notes. With no important comparative component, this work ends up as somewhat narrow cultural history—but a devastating criticism of the antiecclesiastical "power of positive thinking."

JOHN C. BURNHAM
Ohio State University

JAMES C. WHORTON. *Crusaders for Fitness: The History of American Health Reformers*. Princeton: Princeton University Press. 1982. Pp. 359. \$19.50.

Reform thought in both the Jacksonian and Progressive periods was a cord composed of many strands. James C. Whorton has chosen to examine the strand concerned with improving the individual and collective health of Americans. The colorful reformers involved were not, Whorton maintains, "the dolts of popular stereotype" (p. 8). They were "true believers" with a basic commitment to the goodness of nature, the discoverability of natural laws, and the perfectability of the human race. Largely as the result of dramatic personal experiences, they became propagandists for lifestyle changes that, they were convinced, would bring a longer and richer life, free from disease and pain.

The changes advocated involved the ways Americans ate, drank, exercised, bathed, breathed, dressed, thought, related to sex, and defecated. Since most of these reformers believed in the inheritability of acquired characteristics, their messages were delivered with religious fervor; they were cooperating with God (or nature) to bring on the millennium.

No aspect of "health reform" seemed more important than what Americans ate, although both the amount of food consumed and the speed with which it was eaten were of concern. Generally vegetarians

of one type or another, the reformers supported man's "original" Edenic diet with religious, philosophical, and physiological arguments. Conscious of the scientific discoveries of their day, they made good use of those aspects that seemed to support their own views while conveniently ignoring contradictory evidence.

Although a number of the reformers had medical training, they were generally antagonistic to conventional medicine, which they saw as dedicated to the treatment of illness rather than the promotion of health. Since their antagonism was reciprocated by the doctors, the resulting literature was hardly moderate, although both doctors and reformers proclaimed devotion to temperance.

Herein lies one of the major problems confronting the historian. Emotionally charged sources take skillful handling. In addition, the lifestyle changes advocated are still a subject of controversy—and they impinge on each of us. It is not easy to recreate the ideas and spirit of the health reformers with sympathetic objectivity. Whorton makes a valiant effort that in my grade book merits a solid B plus.

The prose in *Crusaders for Fitness* is lucid, the quotations apt and vivid, the individuals discussed representative of, and influential in, the movement as a whole. Whorton is at his best in describing the personality and views of individuals on whom he has done the most extensive primary research, such as William A. Alcott and Horace Fletcher.

In other cases his personal prejudices crop out. One example must suffice. Is it really fair to accuse Ella Eaton Kellogg of "some of the most frightful sins ever committed in a kitchen" for "such crimes as boiling pasta an hour" (p. 209), when a careful reading of the source cited says something quite different? Mrs. Kellogg, actually quite well trained in the new science of home economics, was dealing with a pasta quite different from that found in today's supermarket. She *actually* said it should be "cook(ed) until tender. The length of time may vary from twenty minutes, if fresh to an hour if stale."

This caveat aside, Whorton has given us the most comprehensive coverage of American health reformers to date. With his help we can better appreciate the roots of the modern physical fitness obsession and better understand the social and intellectual milieus of Jacksonian and Progressive America. There is just enough room for argument on some of his points to assure continued study and discussion that is, after all, the life blood of the profession.

RICHARD W. SCHWARZ
Andrews University

JAMES OAKES. *The Ruling Race: A History of American Slaveholders*. New York: Alfred A. Knopf. 1982. Pp. xix, 309. \$16.95.

The Ruling Race could hardly be more ambitious. In this his first book, James Oakes boldly seeks to take on Eugene D. Genovese, this generation's premier interpreter of Southern slaveholders. While Genovese perceived a South dominated by a planter class whose world view was conservative, hierarchical, patriarchal, and paternalistic, Oakes argues that slaveholders, and the world they made, were vastly different. Oakes admits that a conservative culture existed in the South in the eighteenth century (not because of slavery but because immigrants brought with them certain English ideas and habits) but contends that this was a dying world, not an emerging one. In the nineteenth century, slaveholders adhered to an ideology of egalitarianism, liberalism, and individualism. They were antiaristocratic in temperament, evangelical in spirit, guilt-ridden in conscience, and racist to the core. Rather than resisting notions of social mobility, material acquisition, economic fluidity, and equal opportunity, they celebrated them. By the Civil War, slaveholders reflected perfectly the dominant forces of political democracy and a market economy. Slavery had not created a distinctive ideology; instead, slaveholders swam in the middle of the American ideological mainstream.

Fundamental to Oakes's analysis is the undeniable fact that the typical slaveholder was not a planter. Of the nearly 400,000 Southerners who owned slaves in 1850, the majority had five or fewer. And it is the experience of slaveholders who were not planters that is Oakes's principal subject. This "slaveholding middle class" has not until now had its historian. Oakes's description is based on enormous archival research, and although not persuasive on all points, it is often impressive. In *The Ruling Race*, Oakes becomes the Frank L. Owsley of the slaveholders, defending and illuminating these plain masters who have been lost in the shadow of the planters.

Problems arise when Oakes argues that this "small slaveholding culture" was the "(dominant slaveholding culture)." Dominance cannot be established by typicality. Yet, despite his own admonition, Oakes assumes the power of small slaveholders from their sheer numerical superiority. He does not ignore large slaveholders entirely, but he argues (incorrectly I believe) that the aristocrats were few, that they lived only in pockets on the periphery of the South, and that they were isolated and alienated from the slaveholding majority.

In every Southern state, however, planters wielded disproportionate power—economic, political, social, cultural, and ideological. The small yeoman farmer who worked his few acres shoulder to shoulder with a slave or two shared little, other than membership in the "master class," with the wealthy rice barons, sugar lords, and cotton patriarchs. Oakes demonstrates that deep class divisions existed

among slaveholders and not simply between those who held slaves and those who did not. Any attempt to refute Eugene Genovese's complex interpretation of the antebellum South must confront directly the crucial issues of class and power, and this Oakes does not do.

JAMES L. ROARK
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St. Louis*

DONATHAN C. OLLIFF, *Reforma Mexico and the United States: A Search for Alternatives to Annexation, 1854–1861*. University: University of Alabama Press. 1981. Pp. viii. 213. \$21.50.

Reforma Mexico and the United States meets a long-standing need for a reliable study of diplomatic relations between the United States and Mexico in the middle and late 1850s. Grounded on research in the public archives of both countries, it provides a satisfactory analysis of the Gadsden, Forsyth, and McLane-Ocampo treaties, the Churchwell protocol, and other negotiations defining American relations with both recognized and contending political elements during a period of almost continuous civil war and government turbulence in Mexico. It clarifies fluctuations in U.S. diplomacy, and integrates the activities of entrepreneurs like Carlos Butterfield in the overall evolution of American and Mexican foreign policy.

Donathan C. Olliff contends that Mexican liberals, particularly modernization-bent *purros*, sought partnership with the United States, inviting American investment in Mexican development and even soliciting a protectorate status under their northern neighbor. Their purpose was in part pragmatic; liberals believed American loans, investment, technology, and protection might stave off conservatives as well as impede French or Spanish intervention in their debt-ridden country. Many liberals, however, who had experienced exile in the United States, genuinely admired American progress (they had a particular fascination, Olliff explains, with sewing machines) and republicanism, and wanted to reconstruct their nation on the American model. Liberal leaders emerge as rather savvy diplomats, particularly in 1859 when Benito Juárez's government manipulated the United States into recognizing its legitimacy without land cessions that President James Buchanan had deemed a recognition prerequisite. Washington, unfortunately, never capitalized on liberal receptivity, creating a power vacuum later filled by France. Primarily Olliff blames the seemingly always inept Buchanan for the missed opportunity. Consumed by imperialistic designs, the president never comprehended commercial expansion and failed to champion agreements giving the United States valuable transit, trade, and investment

concessions secured by more perspicacious American negotiators.

Olliff's focus on liberal diplomacy, entrepreneurship, and treaty negotiations precludes a fully comprehensive treatment of U.S.-Mexican affairs. Mexican conservatives, even Félix Zuloaga, whose regime the United States recognized for a time, remain a shadowy presence, their doctrines and intent inadequately explained. Texas-Mexican border problems, including slave fugitives and filibustering, receive scant attention; there is no mention whatsoever of Juan Cortina, James H. Callahan, Henry A. Crabb, or peonage. Olliff seems unaware of the ties linking such American adventurers as Chatham Roberdeau Wheat and John S. Ford with liberal revolutionary activity, a deficiency that might have been alleviated by reference to a more substantial number of non-governmental manuscript collections. American racial attitudes toward Mexicans, acknowledged as a fundamental constraint on U.S. policy, escape full consideration.

Still, Olliff provides the best treatment to date of U.S.-Mexican relations during the Reforma. Thanks to this volume and Thomas D. Schoonover's important 1978 study about Mexican-American relations in the 1860s, *Dollars over Dominion*, historians now command impressive scholarship delineating the ideological, commercial, and political ties linking Mexican reformers and American "laissez-faire liberalism"—as Schoonover puts it—in the mid-nineteenth century.

ROBERT E. MAY
Purdue University

NEAL HARLOW. *California Conquered: War and Peace on the Pacific, 1846–1850*. Berkeley and Los Angeles: University of California Press. 1982. Pp. xvii, 499. \$19.95.

In his richly documented narrative of California events during the Mexican War and immediately thereafter, Neal Harlow argues that the conflict was "waged by a process of continuous improvisation, in which distance, time, personality, and coincidence played heavy roles." As its subtitle indicates the bulk of this book concerns the four years, 1846–50, but the author necessarily traces the prewar history of the Pacific coast, covering the Spanish and Mexican eras in California.

United States involvement there produced many dramatic incidents detailed here, most of them well known to historians. Harlow explains Commodore Thomas ap Catesby Jones's invasion fiasco of 1842, mounting frictions among *Californios* and with Mexico in that critical decade, and the perilous influx of foreigners, as well as American suspicion, mostly unfounded, of British and French motives.

Although readers may expect more background

on some of the many figures presented here, Harlow is direct and definite in evaluating American invaders as well as California leaders. He is fair, admitting that the tough and impulsive Commodore Robert F. Stockton was able to turn navy men into effective horsemen and treat conquered *Californios* liberally, while the ill-balanced, insubordinate Captain John C. Frémont, inglorious in his Bear Flag conduct, could command unruly immigrants, mold his volunteers into a devoted battalion, and, without his superiors' knowledge, compose at Cahuenga a wise peace for California.

The author obviously approves of the late-arriving General Stephen W. Kearny, whom he considers endowed with restraint, good judgment, and pertinent experience. Colonel Richard B. Mason and General Bennet Riley he also judges highly as beneficial to America's policy of conquest and the welfare of residents. Despite profound problems caused by confusing orders and a military government whose authority was questionable once the war had ended, they managed California's finance, trade, Indian affairs, and communications reasonably well, while their servicemen were deserting to seek newfound gold. In the nativist Southern California uprising, 1846–47, Harlow allotted considerable blame and discovered no great heroes on either side.

The book's last section covers movements for statehood. When as a result of the national slavery controversy, Congress failed to provide civil government, Governor Riley courageously allowed Californians, mostly newcomers, to create their own state constitution. By the end of 1849, California had been "Americanized" militarily, politically, judicially, economically, and socially.

Elements of surprise, several unsolved mysteries, and absorbing interactions of strong—but seldom wise—men representing different ethnic groups, armed services, and diplomatic stances offer readers varied and vivid hues, which Harlow, through his artistry of clear narration, allows to appear naturally. His more than forty years of familiarity with the best sources are obvious in this skillful presentation. The several repetitions tie up loose ends and avoid confusion.

Although there are few new facts or novel interpretations, this volume is an important survey of a critical era. It unites what many manuscripts and monographs have presented in isolation. Despite enormous complications and factual details, this work is readable, reliable, and useful.

JOHN E. BAUR
California State University,
Northridge

DARLIS A. MILLER. *The California Column in New Mexico*. Albuquerque: University of New Mexico

Press, with the cooperation of the Historical Society of New Mexico. 1982. Pp. xvi, 318. Cloth \$19.95, paper \$9.95.

For the first time in the history of the United States, the Civil War provided hundreds of thousands of Americans, both Confederate and Union, with the opportunity to compare life in their home counties and states with what they experienced in other locations. Many Confederate veterans from Tennessee liked what they saw in Texas and moved to that state following the conflict. Similarly, a number of Union troops from Iowa, attracted by the cheap land and salubrious climate conditions they discovered while serving in Louisiana, founded there the towns of Vinton and Iowa after the war.

It is a similar movement of veterans of the California Column to New Mexico, after being sent to that territory in 1861 to expel the Confederates and fight Indians, that is the subject of this well-researched, albeit at times disjointed, book by Darlis A. Miller, an associate professor of history at New Mexico State University. A specialist in territorial history and the frontier era, the author briefly describes the often-repeated story of the wartime actions of the California Column and its commander General James Carleton, who, after arriving too late to participate in the Battle of Glorieta Pass, encountered but a few Confederate stragglers. The major war activity of the column was the conduct of campaigns against the Apaches and Navajoes in their efforts to establish a strong Federal presence in New Mexico and secure the West for the Union. There is scant mention of the movement of the Navajoes to the Bosque Redondo.

The bulk of the book is devoted to the postwar careers of the 340 troopers who were mustered out of Federal service in New Mexico during the final months of the Civil War and who, following their great adventure, felt they had more propitious economic, social, and political opportunities in the territory than in their native California. It is Miller's contention that, subsequently, the veterans made significant contributions to the further development of the territory and had a decidedly positive impact on the postwar history of New Mexico.

Many of the Californians were a violent breed and were active participants in the numerous conflicts that plagued the territory in the sixties and seventies. Some were partially responsible for provoking the Horrell and Lincoln County Wars along with the El Paso Salt War. One was responsible for the killing of Chief Territorial Justice John P. Slough in 1868. Others were extreme racists who looked with disdain and contempt on the *Hispanos* and Indians despite the fact, as Miller is quick to point out, that some married Hispanic women.

By nineteenth-century Anglo standards, beliefs,

and values, the Californians with their mechanical, business, and political skills did make contributions to their concept of progressive development of the territory in many categories of activity. Like so many other Americans in all sections of the nation, however, they were often corrupt in this particularly corrupt period of history. Among them were those who were as inclined to exploit the Hispanic population as their Missouri and Texas predecessors and contemporaries.

The major fault of this book is that Miller lets these people off too easily. She writes extensively of their "contributions" from an Anglo perspective. Although she does mention the violence and injustice for which they were in part responsible, she fails to present the *destructive* impact of the Californians from an Hispanic perspective.

Despite this omission the book remains a solid contribution to the socioeconomic and political history of the region and deserves to be perused by scholars of the middle period of U.S. history.

ALVIN R. SUNSERI
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STEPHEN Z. STARR. *The Union Cavalry in the Civil War. Volume 2, The War in the East from Gettysburg to Appomattox, 1863-1865*. Baton Rouge: Louisiana State University Press. 1981. Pp. xv, 526. \$30.00.

Classic is a word often used to describe studies in Civil War military history. It is inevitable that Stephen Z. Starr's trilogy, *The Union Cavalry in the Civil War*, will be called a classic when it is completed. After two volumes that description seems to be clearly justified.

In volume 2, subtitled *The War in the East From Gettysburg to Appomattox, 1863-1865*, Starr focuses on the Union's assumption of cavalry supremacy over the Confederacy. Furthermore, he believes cavalry contributions to the Union victory were critical. In the preface he refers to the horsemen in blue as the "cutting edge of the Union Army." The central theme of volume 2 is the honing of that "cutting edge" by Phil Sheridan and his lieutenants.

This work necessarily spends a great deal of time examining Sheridan in and out of battle. Starr agrees with "Little Phil's" superiors who said he "was worth his weight in gold." Yet Starr is quick to criticize Sheridan when criticism is deserved. Of Trevilian Station the author concludes: "The campaign as a whole was undoubtedly a failure and it was absurd of Sheridan to term it a 'constant success.'"

Sheridan's lieutenants were a colorful and varied lot, and Starr brings them alive. You never have to guess where the author stands as this assortment of good and bad generals rides through his pages.

There was Judson Kilpatrick and his "inordinate ambition," the "lush eloquence" of George A. Custer, and the "willful, intractable and unpredictable" W. W. Averell.

This volume is more than a study of battles; it also examines the administration of the Union cavalry's three hundred thousand men and more than four hundred units. Starr's style is sprightly in dealing with battles and clear in his discussion of cavalry administration. Problems in securing mounts, the vital decision to employ Spencer carbines, and the increasing use of troopers in dismounted action are all well developed. So is that nastiest aspect of the conflict—guerrilla warfare.

Starr has found his way to all the sources and handles them effectively. He is realistic in his use of memoirs and informs his readers: "Union and Confederate officers, high and low, tended to recall the events of the war, and particularly their roles in it, through a mythopoetic haze, with, more often than not, a generous dose of self-justification thrown in."

Surely no one knows more about Civil War cavalry than Stephen Z. Starr, and everyone interested in the war eagerly awaits the third volume on Union cavalry in the West.

JAMES P. JONES
Florida State University

CHARLES B. STROZIER. *Lincoln's Quest for Union: Public and Private Meanings*. New York: Basic Books. 1982. Pp. xxiii, 271. \$17.50.

The best psychoanalysts are those with a moral sense of human fallibility but also a deep apprehension of how rough the road to self-acceptance can be. Both skepticism and concern should serve the psychobiographer as well. Yet, like the quantifier who loves software more than conclusions, the psychohistorian too often lets the theoretical mechanics get in the way of genuine understanding. Charles B. Strozier is well versed in psychology. But he treats the Civil War president—indeed the age, too—as a test case for Heinz Kohut's theory of the maturing self, latest psychohistorical fashion in these post-Eriksonian days.

This evaluation by no means indicts the whole book. In the opening chapters, Strozier offers a lively portrayal of Lincoln's early years. He stresses Thomas Lincoln's solid qualities as citizen, provider, and father to an all-too-competitive son. The old farmer of Pigeon Creek impressed contemporaries as something less than the Roman worthy we find here, but Strozier is more convincing about the maternal gifts of Nancy Hanks and Sarah, Abraham's stepmother. By far the best chapters of the book concern Mary Todd and her relation to her

husband. So sensitive is this section that Strozier may be well advised in future to track his psychological prey on the female rather than male side of the sexual divide. He sees her as a complex figure whose compulsions inhibited but did not destroy Lincoln's affection for her. A chapter on Robert Todd Lincoln and his troubled attitudes toward his parents seems well researched. The final chapter on Lincoln's depressions offers a plausible explanation: early loss of his mother, among other experiences, probably shaped a perfectionist mentality that nourished extravagant ambitions, soon dashed. Mary Todd, burdened with her own problems, must have found it hard to cope with her husband's bouts of hopelessness, especially because he used humor as counterpoise. Strozier does well with the Lincolns' domestic life.

Nonetheless, throughout the book, the author fitfully plunges into bathos, a temptation to which admirers of Eriksonian prose are peculiarly attracted. For instance, Lincoln's "ego identity—his accrued confidence in an inner sameness and continuity—was fragile at best" (p. 41). More seriously, the author struggles clumsily with an interpretive horror that lurks under the penultimate chapter title: "The Group Self and the Crisis of the 1850s." According to Strozier, the same forces affecting the domestic Lincoln may be discerned in the national psyche. But society or, to use his fallacious oxymoron, the "group self," has only an external existence with no individualistic drives related to problems of birth, nurture, sexuality, or death. This sentimental diversion detracts from the concluding chapter that convincingly treats the president's growing maturity in office. On balance, Strozier remembers that the biographer, like the good analyst, must see the person whole rather than let theory be the arbiter. After all, psychological abstractions are only a means to that greater aim. But whenever he abandons cold-eyed realism touched with empathy, he lessens the value of what comes close to being a most satisfying contribution to Lincoln studies.

BERTRAM WYATT-BROWN
Case Western Reserve University

THOMAS REED TURNER. *Beware the People Weeping: Public Opinion and the Assassination of Abraham Lincoln*. Baton Rouge: Louisiana State University Press. 1982. Pp. xvi, 265. \$27.50.

Thomas Reed Turner believes that popular outrage over the first assassination of an American president, occurring after four years of bitter civil war, affected the aftermath of the murder and caused many controversial actions. He bases his opinion heavily on newspapers and also uses personal and archival manuscript materials, published sermons,

and a wide range of secondary sources. Cartoons and other illustrations give additional support. Turner tells the familiar story of how Lincoln's murder temporarily ended Northern desire for sectional reconciliation and induced general fury. He then argues convincingly that the government authorities did not create but instead shared a widespread belief that the Confederates were responsible for the assassination. Parenthetically he rejects the frequent claim that almost all Southerners abhorred the murder, but, unfortunately, he offers only a limited array of evidence on this point. He presents Secretary of War Edwin M. Stanton as functioning amidst perceptions of a wide-ranging Southern plot, acting in the main responsibly, and ably organizing the pursuit and capture of those suspected of involvement in the assassination.

With John Wilkes Booth having been killed by his pursuers, the majority of the Northern people accepted a military court as appropriate to try his collaborators in the murder of their wartime commander-in-chief. Evidence introduced before the tribunal seemed to confirm the complicity of Confederate officials. As to this, the author effectively deals with the perjury of several witnesses and, more important, with the mingled gullibility and vindictiveness of Judge Advocate General Joseph Holt, who had procured their testimony. Turner also analyzes closely the cases against the actual defendants. While demonstrating the preponderantly convincing quality of the evidence, he offers valuable information on the use of possible co-conspirators as witnesses and on their post-trial relationships with government figures. Especially useful is the good summary of the controversy between Holt and President Andrew Johnson as to whether the latter was aware of a petition for clemency for Mrs. Mary Surratt. Turner suggests that both men possessed selective memories. Moreover, he makes the important point that much of the sentiment favoring clemency for Mrs. Surratt stemmed less from doubt about her guilt than from a sexual bias against hanging a woman. He concludes that a civil trial at the time would most likely have resulted in a verdict similar to that by the military commission—thus minimizing the possible effect of the biases of some of the military judges and the significance of the required unanimity for a jury verdict. There is also brief discussion of the later trial of John Surratt.

Turner claims that historians have ignored the contemporary context of the assassination and its aftermath. Treating evidence in a vacuum and twisting it to fit preconceived theses, they have produced fanciful accounts of monstrous conspiracies. By his sweeping use of the word "historians," Turner might seem to include among such theorists the great majority of Civil War scholars who have in fact referred to such conspiratorial concepts only to

reject them. As Turner's own historiographical discussion indicates, a relatively small group of writers and film-makers has been responsible for propagating most of the notions to which he objects. The weaknesses of their methods and conclusions are revealed in the Turner book's excellent critical canvass of the assassination literature. Regrettably, this substantial book will reach few in the multitude misled about the Lincoln assassination by sensational films and publications. But it is to be hoped that history teachers will read it and administer its facts as an antidote for the myth-mongers' fantasies.

FRANK L. BYRNE
Kent State University

EDMUND L. DRAGO. *Black Politicians and Reconstruction in Georgia: A Splendid Failure*. Baton Rouge: Louisiana State University. 1982. Pp. xii, 201.

HOWARD N. RABINOWITZ, editor. *Southern Black Leaders of the Reconstruction Era*. (Blacks in the New World.) Urbana: University of Illinois Press. 1982. Pp. xxiv, 422. Cloth \$27.50, paper \$9.95.

In *Black Politicians and Reconstruction in Georgia: A Splendid Failure*, Edmund L. Drago accomplishes several objectives. He illuminates the difficulties black politicians had in obtaining political power and in providing effective leadership; also, he indicates the lack of support from fellow white Republicans, as well as the unrelenting efforts of white Democrats to regain control.

During the Civil War, blacks faced many obstacles. Neither General William T. Sherman, nor his troops, ever saw themselves as abolitionists; his march through Georgia, however, caused over nineteen thousand blacks to leave the plantation to follow him. The treatment they received was harsh. Indeed, the general's prejudices and refusal to enlist black soldiers for fear of alienating Union sentiment caused Henry M. Turner to label such action as "Shermanized officers."

To overcome such prejudices, blacks established churches and an educational movement. It was from these that black leaders emerged. Thus, most of the early leaders of Reconstruction were minister-politicians, whose belief in the brotherhood of man—even that slave owners were created in the image of God and deserved mercy—created their conservative philosophy. The turbulence of Reconstruction, especially the expulsion of black members in 1868, caused these politicians to become more radical and to realize that whites had no intention of being fair. Although Reconstruction was short-lived in Georgia—lasting until 1872—blacks had some success within this "splendid failure." They gained considerable leverage in shaping the postbellum

labor system and learned that to retain political power they would have to be more self-reliant. They failed to gain land redistribution, however; nor was unionization established.

In *Black Politicians and Reconstruction in Georgia*, Drago argues that the black Reconstructionists were not as wealthy nor as numerous as in South Carolina and Louisiana but "stood between the white former ruling class and black peasantry but much closer to the latter" (p. 37). Of the 69 legislators—comprising approximately 20 percent of the legislature—the most prominent were Aaron A. Bradley, Tunis Campbell, and Henry M. Turner. On the county and municipal level black office-holding "remained minimal" (p. 79).

Howard N. Rabinowitz's *Southern Black Leaders of the Reconstruction Era* is an edited volume that examines the question of *how* Southern black leaders functioned in Reconstruction politics and within the Republican party by looking at leaders on the national, state, and local level. The first group, congressmen, includes Blanche K. Bruce and John R. Lynch of Mississippi, Josiah Walls of Florida, James Rapier of Alabama, and James O'Hara of North Carolina. They had similar legislative demands—education reforms, civil rights, and economic securities—but established varied patterns of alliances and relationships with whites. Although O'Hara gave the black community rhetoric and symbols, he built the strongest black political base in the "black second Congressional District" of North Carolina. Most were not natives of the states they served, but practically all acquired land and some wealth during and after their careers. Most of these men—both congressmen and state and local officials—were from an elite group. A large percentage were antebellum free persons of color and had an "essential base in the black community."

The "Collective Biography of State and Local Leaders" comprises the second section of this book, "Individual State and Local Leaders" the third part, followed by the section, "Afterword: New Perspectives on the Nature of Black Political Leadership During Reconstruction." Within the collective biography section, Richard L. Hume provides an informative and balanced section, while David Rankin overly concerns himself with "phenotypes" and calls almost every Afro-American with some wealth a politician—dubiously labeling them as "ward officers." Hume places the constitutional convention delegates in several categories: (1) farmers, (2) ministers, (3) skilled laborers, (4) business and the professions. Hume fails to mention P. B. S. Pinchbeck as acting governor (p. 145, n. 46), however, he adequately constructs the hostile environment in which over 10 percent of these men "were actually victims of klan-type attacks." Richard Chesson's article informs us that Reconstruction in Richmond

was not as black and corrupt as some thought. The black councilmen—all were elected from Jackson Ward—did not have the important committee assignments, nor were they accused of any scandals. Although they never numbered more than seven on a forty-eight member council, they were able to build a black school, establish night school for day laborers, provide coal for indigent blacks and whites, and construct better streets and lights. Thomas Holt found that South Carolina black politicians had a high turnover rate, confronted by both unifying issues (social services, civil rights, and public education) and divisive ones (regulation of the new farm labor system and reform of land ownership).

The section on "Individual State and Local Leaders" gives us a glimpse of local politics. Through such agencies as the church, Freedmen's Bureau, school system, and the American Missionary Association, the rise of such capable, shrewd, and progressive family men as Holland Thompson of Montgomery, Alabama, Aaron A. Bradley and William Finch in Georgia, Dr. Benjamin A. Boseman, Jr., in Charleston, and George T. Ruby of Texas are discussed. On the state level they spoke for civil rights legislation, educational reforms, and economic development, while on the local level they promoted social welfare issues, adequate street improvements, and support for local education. Three of these leaders—Ruby, Boseman, and Thompson—died in their forties, and all left politics because of personal tragedies or violence, factionalism, or the unwillingness of the national Republicans to support their efforts.

The "Afterword" raises many new and challenging questions, although it also exhibits shortcomings. There are summary statements and comments on the directions and implications for new studies, but the whole weakly implies that most of the failure of Reconstruction was due to black factionalism without noting the violence that accompanied this era. One is left with the impression that slavery had not existed, nor had the brutal racist Ku Klux Klan and other groups. Perhaps the comments of Drago's book for Georgia could best apply to the entire South (and nation): "the black politicians were the special target of Klan violence" (p. 93); "many whites continued to act as if slavery had never ended" (p. 112); "to crush their opponents, the conservatives employed demagoguery, social ostracism, intimidation, fraud, violence, and the Ku Klux Klan" (p. 144); "racism prevailed all classes of white Georgians" (p. 102).

There are omissions in both volumes. Although amply documented for the most part, the addition of maps would have been helpful. Drago's dubious statement that Georgia's black Reconstructionists stood closer to the black peasantry than did black

Reconstructionists in Louisiana needs documentation (p. 45). In *Southern Leaders of the Reconstruction Era*, there are several typographical errors (p. 39), and the date on the *New Orleans Tribune* should be 1865 insted of 1864 (p. 171).

All in all, these new volumes will add to our knowledge of Reconstruction, which, as Edmund Drago reminds us, is still best described by W. E. B. DuBois as a "splendid failure."

CHARLES VINCENT

Southern University and A&M College

DAVID M. DEAN. *Defender of the Race: James Theodore Holly, Black Nationalist Bishop*. Boston: Lambeth Press. 1979. Pp. 150. \$14.95.

This excellent biography of a pioneer black nationalist is carefully researched. It reveals the frustrations and hopes of the first black bishop consecrated by the Episcopal church.

James Theodore Holly was born on October 3, 1829 in Washington, D.C. By age twenty-one, Holly had come to embrace a black nationality and to support the black emigration movement. Although initially interested in emigrating to Liberia, he later settled in Canada and finally in Haiti. During the 1850s Holly was active in a number of colonization projects.

In 1852 Holly renounced Roman Catholicism and was confirmed in the Protestant Episcopal church. He was ordained deacon on June 17, 1855, and priest on January 2, 1856. Holly was the fourteenth Afro-American to take ordination vows in the church.

After 1854 Holly came to believe that the best hope for black Americans to achieve personal freedom was to separate themselves from whites. He advocated Haiti as the logical place to accomplish this segregation. After a brief visit to Haiti in 1855, Holly and the 101-member "New Haven Pioneer Company of Haytian Emigrants" sailed for Haiti on the *Madeira* on May 2, 1861. Holly remained in Haiti for the remainder of his life, visiting the United States seven times between 1862 and 1902.

Unfortunately, within a year of his arrival in Haiti, Holly's dream of a successful mass emigration of blacks to that country had ended. The threat of military disturbances, the failure of the government to provide promised land, and the reports of sickness and death among the colonists had discouraged the flow of emigrants.

As early as 1862 Holly had petitioned the general convention of the Protestant Episcopal church for a bishop in Haiti. He began the formal organization of the church in Haiti with the establishment of Holy Trinity Church in Port-au-Prince in 1863.

Over time he assumed more and more administrative power.

On November 3, 1874, in New York City, the House of Bishops of the Protestant Episcopal church elected James Holly the first bishop of Haiti. With his consecration Holly became the first black Episcopal bishop. Three weeks after returning to Haiti, Holly was elected bishop of an independent Haitian church, the Orthodox Apostolic church.

Holly's life was one of setbacks. He never received enough salary from the Domestic and Foreign Missionary Society to take care of his needs. He was always plagued by indebtedness. Despite his best efforts, the Orthodox Apostolic church was often attacked from inside and out.

James Theodore Holly, as one of his sons recorded, was "a race man in every possible sense." Of the prominent black leaders favoring emigration in the 1850s, Holly was the only one who emigrated permanently. As the author emphasizes, he was a living link between the crusaders of the 1850s in support of emigration to Africa or the West Indies and the supporters of the Pan-African movement of the early twentieth century. After fifty-six years in the ministry, almost fifty years of missionary work in Haiti, and over thirty-six years as the first black bishop of the Protestant Episcopal church, Holly died on March 13, 1911.

The book suffers from several shortcomings. Dean is inaccurate in stating that Holly was the second black bishop of any major white Christian church in the world. Francis Burns was elected the first missionary bishop of the Methodist Episcopal church on October 14, 1858 and John Wright Roberts was elected on June 20, 1866. Additionally, there should have been more in-depth discussion of the Protestant Episcopal church's image of the capabilities of black missionaries. Finally, it would have been revealing to learn the reason for the church's apathy toward the Haiti mission, specifically, why there was a difference in church attitude toward missions in Liberia and Haiti. The author only touches on these issues. Despite these omissions, however, Dean has made a valuable contribution to the history of black nationalist thought.

SYLVIA M. JACOBS

North Carolina Central University

JAMES PICKETT JONES. *John A. Logan: Stalwart Republican from Illinois*. (Florida State University Book.) Tallahassee: University Presses of Florida. 1982. Pp. xii, 291. \$20.00.

In recent years, almost all the leading personalities of Reconstruction and the Gilded Age have become the subject of biographies; the post-Civil War career of John A. Logan, however, long remained unwrit-

ten. This gap has now been filled by James Pickett Jones, the author of *Black Jack: John A. Logan and Southern Illinois in the Civil War Era*. His *John A. Logan: Stalwart Republican from Illinois* completes his study of the soldier-politician.

Like its 1967 predecessor, the present book is a carefully researched, workmanlike, well-balanced treatment of the subject. The author traces Logan's career from his 1866 election as Illinois congressman-at-large on the Republican ticket, through his 1871 elevation to the Senate, to his death in 1886. Focusing on the former Democrat's political activities, the book stresses the general's role as the organizer of a stalwart political machine in his home state and his claims to leadership within the Republican party. Although disappointed in 1880 by the failure of the movement to win a third term for General Ulysses S. Grant, Logan was so popular with veterans and had so efficient an organization that he became a candidate for the presidency in 1884. Even though he failed to win the nomination and had to content himself with second place as the running mate of James G. Blaine, who lost the election, he remained undaunted. Conducting a skillful campaign for re-election to the Senate in 1885, he had hopes of higher office in 1888, but died before he could realize his ambition. His long-time leadership of the GAR, his inauguration of Memorial Day, and his colorful, bloody-shirt oratory made him a well-known figure in post-Civil War America.

Because Logan's later career lacked some of the excitement of his earlier life, it is perhaps inevitable that this volume is less dramatic than its predecessor. After all, whether or not, as the author maintains, Logan was an idealist tempered with political pragmatism, there is no lasting monument to his labors other than the celebration of Memorial Day. No important bill bears his name; no significant measures for the benefit of blacks or women can be attributed to him. His contributions were largely made prior to 1867, during the Civil War, when he rallied the Democrats of Southern Illinois to the flag and served with distinction as a volunteer officer. Although the general's relationship with his highly intelligent wife and family is interesting, the author has concentrated largely on Logan's political career, so that the book lacks some of the flair of its predecessor.

There are a few surprising omissions in the text. In view of Logan's position as one of the managers of Andrew Johnson's impeachment trial, the circumstances surrounding this dramatic event might have been considered in a little more detail. Also there is no reference to the Republicans' subsidy to Benjamin F. Butler's independent presidential bid in 1884, surely an important aspect of the Blaine-Logan presidential campaign. But in spite of these

minor shortcomings, this biography of "Black Jack" Logan together with its companion volume, will probably long remain the standard work on the subject.

HANS L. TREFOUSSE
Brooklyn College and
Graduate Center
City University of New York

DAVID J. MURRAH. *C. C. Slaughter: Rancher, Banker, Baptist*. (M. K. Brown Range Life Series, number 15.) Austin: University of Texas Pres. 1981. Pp. x, 173. \$14.95.

C. C. Slaughter rode to success on the peculiar opportunities afforded by his time and place. Born in the newly created Republic of Texas, he was the son of a Baptist minister who combined preaching with raising cattle. Young Slaughter fought Indians and herded livestock on the frontier before the Civil War, and at war's end he reaped windfall profits by pioneering the long trail to cattle markets. By 1872, when he was thirty-five years old, a Dallas paper hailed him as the "Cattle King of Texas," and by the turn of the century he ran some forty thousand head of cattle over more than a million acres. In addition, he held substantial banking and real estate interests in Dallas, from which city he presided like a patriarch of old over his empire and his family of nine children.

In this biography David J. Murrah lifts Slaughter from relative obscurity and places him beside other cattle barons of the period, men such as Henry Miller of California and George W. Littlefield of Texas. Following Lewis Atherton in *The Cattle Kings*, Murrah emphasizes entrepreneurship as the key to Slaughter's success. Free grass and cheap range cattle gave him his initial boost up the ladder, but his business acumen made him a man likely to succeed under any circumstances. Indeed, his enduring success stemmed from his ability to meet changing conditions. Anticipating the end of the open range, he pioneered the use of barbed wire for fencing; and understanding the limitations of the longhorn cow, he introduced blooded stock to west Texas. When drought and plummeting cattle prices drove other ranchers into bankruptcy, Slaughter, wearing his banker's hat, collected the remains to enhance his own domain. When farmers crowded into the range lands, he switched from cattleman to realtor to take advantage of rising land prices.

Slaughter departed from the general pattern of his contemporaries on the range to play an active role in his church. Commenting that "praying cattlemen are the exception and not the rule in Texas" (p. 51), he became a major contributor to Baptist causes. He endowed five church colleges, published

the denominational organ, and helped found a hospital that the trustees tentatively named for him until they accepted the infeasibility of a hospital named Slaughter. That hospital has grown into a major medical center in Dallas.

Murrah points to these philanthropies as Slaughter's monument and suggests that historians have previously neglected the cattlemen because of the lack of primary records. In 1966 one of Slaughter's great-grandsons remedied that lack by depositing a collection of letters at Texas Tech University, where Murrah is archivist. That initial collection led to the discovery of other material, all of which Murrah has used to advantage in this biography. He has written a well-documented, solidly researched study of entrepreneurship in the American Southwest during the late nineteenth century.

MARILYN MCADAMS SIBLEY
Houston Baptist University

JOSEPH A. FRY. *Henry S. Sanford: Diplomacy and Business in Nineteenth-Century America*. (Nevada Studies in History and Political Science, number 16.) Reno: University of Nevada Press. 1982. Pp. xi, 226. \$9.25.

Joseph A. Fry of the University of Nevada, Las Vegas, has written the first comprehensive study of Henry S. Sanford's (1823–91) varying diplomatic and business career. During a forty-year career Sanford played an active role in some of the important developments in nineteenth-century American diplomacy, particularly during the Civil War. In the 1850s as chargé d'affaires in Paris, he joined "Young America" in celebrating "republican simplicity" and ostensibly denigrating the trappings of European monarchy. As United States minister resident to Belgium in the 1860s, he laid the groundwork for the North's surveillance of Confederate agents, coordinated the purchase of valuable war material, directed Northern propaganda efforts, and carried the offer of a Union command to Giuseppe Garibaldi, the famed Italian conqueror of the Two Sicilies. These endeavors alone, writes Fry, made Sanford one of the Union's most important diplomatic representatives. But, following the war, Sanford, according to Fry, became the "central figure" in the evolution of United States policies in the Congo. His aggressive calls for an "open door" in the Congo "constitute one of the most telling examples of this theme in American diplomacy between 1865–1890."

While these activities made Sanford an important figure in nineteenth-century American diplomacy, his significance was not confined to foreign affairs. He also had an active business career. In the 1850s he promoted economic and territorial expansion in Latin America. After the Civil War he became the

most important single contributor to the development of Florida's late nineteenth-century citrus industry.

Fry has written a fine and useful book. His central argument is that over the second half of the nineteenth century, Sanford's diplomatic activities consistently influenced or symbolized the primary trends in United States foreign policy. Fry maintains that Sanford's activities at his post in Belgium made him the equal of John Bigelow in Paris and second only to Charles Francis Adams in England as the North's most important Civil War diplomat. While Sanford never quite attained either the fame or fortune he pursued, his life was not without significance. During his long career "he played a leading role in some of the most fundamental developments in nineteenth-century American diplomacy."

While some of these conclusions might be debated, Fry has bolstered his arguments with extensive research in an impressive array of sources and has written a clear, even-handed, and convincing case for his main points. Fry's book is organized both topically and chronologically. This is a first-rate work with impeccable scholarship. It makes an important contribution to nineteenth-century American diplomatic and business history and will remain the standard work on Sanford for many years to come.

VINCENT P. DE SANTIS
University of Notre Dame

PAUL C. NAGEL. *Descent from Glory: Four Generations of the John Adams Family*. New York: Oxford University Press. 1983. Pp. xiv, 400. \$25.00.

How shall we account for the extraordinary hold of the Adams family on modern American consciousness? Part of the answer, of course, is their prolific output. Fed by many tributaries, the great Mississippi of Adams family papers requires 608 microfilm reels just to bring the story down to 1889. For the so-called "Fourth Generation" (the children of Charles Francis Adams), we have thirty-six reels of Henry Adams papers as well as Charles Jr.'s 3000-page journal, twenty-nine pocket diaries, and twenty-four boxes of "Loose Papers." Without this horde, the job crisis in history would be a lot worse than it is. A small army of scholars labors night and day producing new volumes of Adams letters, diaries, anthologies, biographies, public television scripts, and interpretive monographs. Several remarkable Adams wives—Abigail, Louisa, Clover—have attracted significant scholarly attention in their own right.

But the Adams vogue surely involves more than the mere profusion of materials. In a culture where continuity of achievement across generational lines

is problematic at best, and rare indeed in public life, the Adams record fascinates us. From the 1760s to the 1920s, not a year passed that at least one family member was not prominent in the nation's political or intellectual life. We are attracted, too, by the remarkably consistent Adams outlook. While others succumbed to Enlightenment dreams of perfectibility, Victorian cant about progress, or Progressive visions of advance through science, the Adamsses remained deeply skeptical about the human condition—a skepticism that in Brooks and Henry culminated in cataclysmic visions that from our post-Hiroshima perspective seem chillingly prophetic. And surely, too, in this narcissistic and psychology-saturated era, we respond to the way generations of Adamsses obsessively probed their own emotions, motives, and impulses, and those of their children, siblings, parents, and ancestors.

With so many books by and about the Adamsses already in print, Paul C. Nagel is brave indeed to contribute yet another, particularly one attempting nothing less than a comprehensive history of the entire family from the marriage of John and Abigail in 1764 through the death of great-grandson Brooks in 1927. Such a work could all too easily have become a superficial overview, a sentimental paean, or a confusing genealogical tangle. Nagel does not entirely avoid these risks. There are patches of purple prose, paragraphs of potted history, and confusing sections. (The women—a voluble chorus of Minnies, Elsie, Lizzies, Nannies, Fannys, and Daisys—are particularly difficult to keep straight.) But for the most part, Nagel succeeds in his risky undertaking. After some shaky early chapters, the narrative settles down to a steady and even engrossing pace. One is sorry when the book ends and vaguely resentful of the “Fifth Generation” for producing no presidents, ambassadors, geniuses, or even spectacular reprobates to keep our author going a bit longer. Each chapter focuses on one individual (or couple) with other family members treated in relation to these central figures. A subordinate person in one chapter may then emerge as the dominant figure in a succeeding chapter. The Adamsses were remarkable stylists, and Nagel quotes them liberally, exploiting his treasure trove of material without being overwhelmed by it.

Wisely, Nagel emphasizes those Adams traits that give the family contemporary resonance: the preoccupation with family, the bleak world-view, the introspection. Most biographies of notables revolve around the public achievements or failures, with family matters and the apparent ephemera of life introduced merely to provide “human interest” or a change of pace. Apart from the earnest psychobiographers, those writers who deliberately set out to present the “human side” of a famous personage tend to produce sentimental pastiches.

Nagel avoids this pitfall while decisively reversing the usual emphasis. Even the most momentous “historical” events are relegated to the background, while family concerns and introspection dominate the foreground. And he convinces us that this reflects the Adamsses’ own valuation, even when they succumbed to the lure of the public arena. As Louisa (Mrs. John Quincy) Adams observed: “Families were created before Nations.” Like the diaries and letters on which he relies, Nagel devotes as much space to the assorted Adams alcoholics, ne’er-do-wells, suicides, neurasthenics, and amiable plodders as to the luminaries. Despite the grandiose and somewhat misleading reference to “Glory” in the title, the book’s controlling viewpoint is that expressed in a Charles Francis Adams diary entry: “The history of my family is not a pleasant one to remember. It is one of great triumphs in the world but of deep groans within, one of extraordinary brilliancy and deep corroding mortification.”

Nagel’s account plainly shows the toll of the familial self-scrutiny that underlay the clan’s remarkable achievements. The founding members worried endlessly about what John Quincy Adams called “the blast of mediocrity” in their children. Their rigid insistence on absolute self-discipline and high achievement in their offspring aroused feelings of hopeless inadequacy in all but the strongest. Yet even as they courted public approval they dismissed it as empty and vain, leaving a highly confusing moral legacy to their descendants. By the end of the nineteenth century, with the line clearly running thin, the focus of critical scrutiny reversed itself, as family members now brooded endlessly about the character, values, and reputations of their parents, grandparents, and great-grandparents. Psychologically hemmed in by family, living and dead, these later Adamsses wrote of each other with a viciousness unmatched by any enemy, ranging from Charles Jr.’s off-hand description of his brother Henry as “a damned solemn, pompous little ass” to Henry’s own considered view of John Quincy Adams as “a fraud and hideous moral depravity.” In the lexicon of Adams family discourse, Henry’s comment that his sister Mary “irritates me less than my brothers do” ranks as an impulsive outburst of warmth and good feelings. One closes the book wondering if it was all worthwhile. At the simplest human level, does the record of achievement justify all the psychic pain and “deep corroding mortification”? And this, of course, is precisely the question that haunted four generations of Adamsses.

Some years ago, apropos the publication of John Adams’s youthful diary, Bernard Bailyn called for interpretative studies that would help us make sense of the mass of Adamsiana pouring from the scholarly presses. Nagel has ably contributed to this process of intellectual digestion. A finely crafted biography-

cal study full of unostentatious but perceptive judgments and insights, *Descent From Glory* should appeal to both professional historians and general readers alike.

PAUL BOYER
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EDWARD CHALFANT. *Both Sides of the Ocean: A Biography of Henry Adams, His First Life, 1838–1862*. Hamden, Conn.: Archon. 1982. Pp. 475. \$32.50.

From Henry Adams's death in 1918 until the close of World War II, his life was known mostly through *The Education of Henry Adams*, a deliberately misleading work, and through the attempts by several women to foster a reverent admiration for his memory. Two of these women, Louisa Hooper Thoron and Aileen Tone, had been close to the aged Adams and were known to believe that they had special insight about "Uncle Henry" and to be unhappy with the disinterested approach scholarship took to Adams after 1945. Since then there have been many studies of Henry Adams, the foremost of which is the splendid three-volume biography by Ernest Samuels. The first portion, *The Young Henry Adams*, appeared in 1948, a time when Edward Chalfant's work was beginning. Samuels's trilogy went on to win wide acclaim, including in 1965 the Pulitzer Prize for biography, but it was widely understood that Tone and Thoron were not pleased with Samuels. Now we have a book long in preparation, one devoted to the outlook of these women.

Edward Chalfant's attempt to portray Henry Adams is thus very different from Samuels's. In fact, Chalfant, a professor of English at Hofstra University, does not mention Samuels's biography. Nor does he appear to have used consistently the wealth of relevant manuscript and secondary sources that has appeared since he began his work, over forty years ago. Instead, Chalfant emphasizes his debt to Tone and Thoron for helping shape his view of Henry Adams and for assisting in preparing the early drafts of this book. The book is dedicated to Tone. It is the first of a trilogy in which Chalfant intends to portray Henry Adams as having three lives or careers, any one of which would have brought distinction to ordinary persons.

Concentrating in this volume on six years of Adams's career, 1856–62, Chalfant takes nearly four hundred pages to deal with a period that Samuels covers in 120 pages. Chalfant's main topics are Adams's student days at Harvard; his two years of wandering and occasional study in Europe; his brief stay in Washington during the succession winter of 1860–61 when his father, Charles Francis Ad-

ams, was in Congress; and his first few months in England serving as an anonymous correspondent for the New York Times when he was also secretary to his father, the American minister. Chalfant contends that during these six years Henry Adams believed he was destined to have an important career, one valuable for his nation and for the world. According to Chalfant, Adams had completed at age twenty-four in 1862 a lifetime of amazing achievement and had fulfilled his own high expectation.

To show Henry Adams triumphing as a leader who made significant contributions, Chalfant advances numerous exaggerations. His approach is typified in this assertion: "Among his preternatural abilities Henry Adams had none more amazing than his ability to foreknow his own future" (p. 157). The greatest blunder is when Charles Francis Adams, Henry's father, is misrepresented. Chalfant makes him into a weak bumbler. This is necessary if Chalfant is to show Henry, aware of his parent's supposed failings, courageously becoming the family leader. Unfortunately for this hypothesis, neither Henry's letters nor his father's manuscripts, including the latter's great diary, support this contention.

Chalfant, however, evidently has used neither C. F. Adams's diary nor his marvelous correspondence from 1861–68 with Henry's eldest brother John, letters unfilmed but available at the Massachusetts Historical Society. These materials, among many others, reveal a strong and talented C. F. Adams, whom Henry respected and followed. This has been the accepted interpretation, but Chalfant contends that such a view was formed, paradoxically, because Henry practiced being self-effacing and withdrawn, so that posterity would not perceive that it was Henry himself who shaped the course of events during 1861–62, even to being crucial in pacifying the Mason and Slidell crisis (p. 379). Chalfant attributes profound international influence to Henry's anonymous reports from England, published in the New York Times, bulletins that Chalfant quotes at great length. He then brings the biography to a startling close, contending that Henry Adams died for his father by selflessly giving C. F. Adams a seeming capacity for wisdom, courage, and decisiveness. "This carefully fostered illusion," Chalfant says, "served successfully to augment his father's stature" (p. 445).

Anyone casually acquainted with C. F. Adams's diary and other family manuscripts must know that this is preposterous, a description that seems to fit most of Chalfant's book. His tall tale about Henry Adams might have pleased Tone and Thoron. It cannot satisfy any reasonable inquirer today. While taking the reader on a long journey into Chalfant's imagination, the book proceeds by many digressions, is often ineptly written, and is marred by

frequent, serious typographical errors. It is grounded on inference and supposition and lacks consistent scholarship and captivating style.

It must be said that Chalfant often qualifies his contentions with terms such as "perhaps," "seemingly," "may have," "suppose," and "if." Also, he concedes in his footnotes something more about his fondness for supposition. For example, after inquiring "If it were asked when he [Adams] conceived *The Education of Henry Adams*—at first glance a very hard question—it could be answered with considerable assurance: why, of course, in May 1860, in Rome" (p. 158), he has the footnote to this assertion state: "His conceiving the *Education* in Rome in 1860—if in fact he did conceive it there and then—might possibly have been inspired by the thought that, in the absence of a book about himself, she [Mrs. John Quincy Adams] would be little remembered" (p. 414, n. 6).

Despite the lifetime of labor behind it, this portrayal of Henry Adams brings us no closer to understanding the subject. It only reminds us of the circle that once blindly adored Adams. Fortunately, the books of Ernest Samuels, J. C. Levenson, Earl Harbert, William Dusinger, and others whose work apparently was disregarded by Chalfant, have opened our way to appreciating the authentic Henry Adams, in whom there is ample cause for admiration.

PAUL C. NAGEL
Virginia Historical Society

CLYDE A. MILNER II. *With Good Intentions: Quaker Work among the Pawnees, Otos, and Omahas in the 1870s*. Lincoln: University of Nebraska Press. 1982. Pp. xiii, 238. \$21.50.

In one of his initial executive actions President Ulysses S. Grant sought to reform the Indian Bureau by appointing members of the Society of Friends to certain of the more troublesome superintendencies and agencies in the field. Among these was the Northern Superintendency that exercised jurisdiction over half a dozen reservations in Nebraska and a small area of northeastern Kansas. Here, under trying circumstances resulting from white land hunger and public insistence that tribal removal was in the best interests of all, the Hicksite Quakers sought to eliminate corruption and improve the government's policy for Indians. To uncover how well they accomplished this is the objective of the three case studies selected by Clyde A. Milner II.

Focusing on the Pawnee, Oto-Missouria, and Omaha tribes, Milner presents much evidence of the Hicksites' overbearing paternalism, their per-

sonal commitment to the destruction of traditional society as a means of saving individual Indians, and their unwavering belief that Christian Indians on fee-simple homesteads was the exclusive solution to the shocking decline of native America. Thus "With Good Intentions" the Hicksite Quakers (like their Orthodox brethren in the even more troublesome Central Superintendency to the South) provided yeomen direction to the ideological trajectory pointing to the Dawes Act of 1887. Indeed, and again with ample documentation, the author suggests that even in the absence of the 1887 legislation the outcome was never in doubt, and that by their day-to-day activities in the trans-Missouri West the Quakers in the 1870s played a leadership role in the ultimate destruction of the Indians' land base. Milner thus joins ranks with Paul Wallace Gates and others who for some time have argued for a definable allotment policy well in advance of 1887.

Particularly tragic was the stultifying ethnocentricity that clouded Hicksite vision regarding inevitable white techniques for appropriating allotments through shoddy leasing contracts, the encouragement of excessive trader indebtedness, and quick sale of land titles following premature and often arbitrary termination of federal trusteeship. In short, controlling blatant corruption in agency administration and simply "being good" were not enough. What happened to the Pawnees who were forced into the Indian Territory, to the Oto-Missourias who were racked with factionalism, and the illusion of Omaha accommodation, were, in large measure, not unlike the experience of the non-Quaker agencies and, from a broader perspective, well within the mainstream of Indian policy dating years prior to Grant's so-called "Quaker Policy."

An otherwise well-developed narrative is occasionally marred by digressions involving unconvincing relationships between native customs and tribal response to government strategy, as, for example, the suggestion that Oto-Missouria naiveté in the face of paternalism is best understood in terms of traditional child-rearing practices. Detracting, yes, but not enough to damage an important analysis of "Quaker Policy" and the veracity of the case-study method for understanding U.S. Indian policy.

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PETER IVERSON. *Carlos Montezuma and the Changing World of American Indians*. Albuquerque: University of New Mexico Press. 1982. Pp. xv, 222. \$17.50.

Peter Iverson has written an excellent biography of Carlos Montezuma, who became one of the most prominent native American leaders during the first

decades of the twentieth century. Montezuma had a remarkable career. Born on the Yavapai reservation in Arizona, he was captured by a group of Pimas and sold to Carlos Gentile, an Italian immigrant. Montezuma was educated in New York and Illinois public schools and encouraged by a Baptist minister to enroll at the Chicago Medical College where he received an MD in 1889.

After he graduated, Montezuma was employed as a physician for the Bureau of Indian Affairs. He also worked for and became a close friend of Richard H. Pratt, who had established the Carlisle Industrial School in Pennsylvania to prepare Indians for assimilation into American society. In 1896, Montezuma returned to Chicago and accepted a position at a medical clinic because he wanted to prove that Indians could become skilled professionals in an urban environment.

Five years later, he made a trip to the Fort McDowell reservation where he rediscovered his people and tribal heritage. In 1911 Montezuma played an important role in organizing the Society of American Indians, which advocated assimilation and criticized the reservation system. More importantly, he helped the Yavapai defend their water and land rights. Montezuma also published the journal *Wassaja*, which advocated the abolition of the Indian bureau. In 1922, Montezuma returned to Fort McDowell to be with his people where he died in a brush shelter.

Iverson provides a favorable assessment of Montezuma's life. He believes that Montezuma demonstrated that Indians could successfully compete in the modern world. Nevertheless, Montezuma was not a white man's Indian. He exposed the incompetence of Indian bureau officials, defended Yavapai tribal dances, and fought vigorously for their water rights on the Verde River.

The author offers a revisionist account of Montezuma's role in the Society of American Indians. He rejects Hazel Hertzberg's unflattering portrayal of Montezuma as a factionalist who spent most of his time attacking fellow "Red Progressives" and convincingly shows that Montezuma was deeply committed to the success of this organization.

This book has a few shortcomings. Not enough attention is given to the psychological impact that occurred when Montezuma was captured and taken from his family. We also need more analysis concerning Montezuma's legacy, for his ideas are very important for understanding both Indian and congressional mistrust of the 1934 Indian Reorganization Act and the postwar terminationist drive to abolish reservations and the Indian bureau.

These criticisms should not detract from the value of Iverson's book. It is well researched, and maps and photographs enhance the text. This volume fills an important gap in our understanding of Indian

history from land allotment to the reform era of the 1920s.

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SANDRA L. MYRES. *Westering Women and the Frontier Experience, 1800–1915*. (Histories of the American Frontier.) Albuquerque: University of New Mexico Press. 1982. Pp. xxii, 365. Cloth \$19.95, paper \$9.95.

Sandra L. Myres's *Westering Women and the Frontier Experience, 1800–1915*, is a comprehensive history of women who crossed the Mississippi River. It touches on the experiences of Anglo, Hispanic, Amerindian, and black women, and although the subtitle specifies "the Frontier Experience" the dates 1800–1915 indicate that the book has a far more ambitious framework. Myres begins with a consideration of the traditional images of Western women. She documents the overland experience, land breaking, homemaking, women's organizational efforts, the suffrage movement, and entrepreneurial ventures. The last chapter on women working in Western states may prove the most valuable of all, for it contains much that is original and useful for future scholarship.

Myres's primary hypothesis is that the Western experiences of men and women did not differ in kind or degree. The frontier "offered challenges" and "provided opportunities" for both sexes. Westering is seen in these pages as a kind of Darwinian training ground for the fit; it made women and men more "adaptable" and better suited for the new life.

Whether or not readers will agree with Myres's general proposition, *Westering Women* succeeds best when it covers subjects within the traditional purview of historical writing. Myres is a competent historian and an indefatigable researcher. Many of the archival sources she has tapped are published here for the first time. The book becomes vulnerable, however, because it is overextended, attempting to present women in all regions of the West, from a cross-cultural perspective, and over more than a century of time. "Women, Race, Religion and Class," for example, are all gathered into a single chapter in which race and religion are superficially examined and consideration of class is hardly discernible. Presenting no quantitative analysis, Myres continually generalizes about "some," "most," and "many" women. And when she concludes that there was, among Anglo and Hispanic women, a "good deal" of visiting, a "good deal" of intercultural "mixing" (an undefined term), or that intercultural marriages were "fairly common," it is difficult to see that such judgments are useful.

Finally, Myres attacks those she calls the "feminist

historians," applying that term as though it were pejorative. "Radical" feminist historians, according to Myres's definition, are those who describe the frontier as imposing especially difficult conditions on women. But the issue is not whether the frontier made "drudges" of women; rather it is whether any historical event or regional condition imprints different experiences upon one sex or the other. In recent years, we have seen a fundamental reshaping of historical thinking based on studies of race, sex, work roles, class orientation, family structure, and life cycle. Little of this theoretical writing seems to have had an impact on Myres's work. Myres does not approve the recent writings about women, but neither does she entertain the larger issues that are raised. As a result, there is a curious sense in which reading *Westering Women* is like reading history written twenty years ago. By virtue of its strengths—and its weaknesses—the book is likely to generate discussion.

LILLIAN SCHLISSEL
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ROBERT L. GRISWOLD. *Family and Divorce in California, 1850–1890: Victorian Illusions and Everyday Realities*. (SUNY Series in American Social History.) Albany: State University of New York Press. 1982. Pp. xii, 254. Cloth \$34.50, paper \$9.95.

Robert L. Griswold, in his book *Family and Divorce in California, 1850–1890*, addresses a critical question that historians have vigorously debated over the last several years: What was Victorianism and who adhered to it? Griswold uses four hundred richly detailed divorce cases from California to explore the lives, values, and marriages of ordinary Americans in the late nineteenth century. His findings are in tune with much recent scholarship: Victorianism was not as austere, authoritarian, and oppressive for women as was previously supposed. His description of Victorianism is solid and convincing, but the interpretation of nineteenth-century marriage that he draws from this evidence is problematic.

Some of Griswold's findings are quite striking. The documents show that sex role expectations for Victorian men and women were quite clearly defined, and that they were endorsed and upheld by men and women from all classes and occupational categories. Women were to be good and responsible wives and mothers, tending to their domestic tasks and children with skill and devotion. As the moral guardians of the home, they were to be chaste and virtuous. Men were expected to provide for their families, to be kind and respectful toward their wives, and to be restrained in their sexual appetites so as not to offend or destroy their wives' innate delicacy.

These role prescriptions added up to what

Griswold terms the "companionate ideal." Because women's rising domestic status softened male authority within the home, he sees the "companionate" marriage as a partnership of equals. It is at this point that his argument rests on shaky ground. Most scholars of the nineteenth century agree that the ideology of domesticity did elevate women's status, especially in the home. But whether this marked a giant step toward equality between the sexes, as Griswold suggests, is still a matter of controversy.

Griswold's illuminating evidence provides a portrait of Victorian marriage in which women were often physically frail and weak, in which they were economically dependent on their husbands, in which they were less than eager for sexuality, and in which they were assumed to have innate maternal instincts and to be content with their domestic role. True, men were not to beat or abuse their wives and were to be kind and loving to them and their children. But these characteristics were not unknown to pre-Victorian families, nor do they necessarily add up to a marriage based on equality. Griswold's use of the term "companionate" to describe these unions is not entirely convincing.

Nevertheless, the issues raised concerning the full implications of the cult of domesticity are provocative. Griswold's evidence does indeed show that the domestic ideal took root deep in the hearts and homes of nineteenth-century Americans. He argues that this development was a critical step on the road toward women's emancipation. Another way of interpreting his evidence is to see that Victorianism was truly alive and well in the late nineteenth century, that women were segregated into a sphere that limited their pursuits to those that were extensions of domesticity, and that in spite of all the efforts of Victorian women to use the code of true womanhood to their best advantage it provided a narrow and confining role from which women are still trying to free themselves. While it is certainly appropriate to move away from visions of nineteenth-century women as victims—and this book does a great deal to contribute to this important task—it is dangerous to err in the other direction by equating companionship with equality, or by celebrating domesticity as an important step toward women's emancipation.

ELAINE TYLER MAY
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DOLORES HAYDEN. *The Grand Domestic Revolution: A History of Feminist Designs for American Homes, Neighborhoods, and Cities*, Reprint. Cambridge: MIT Press. 1982. Pp. viii, 367. Cloth \$19.95, paper \$9.95

While the struggle for sexual equality has been waged most recently in judicial and legislative ven-

ues, parity in the domestic division of labor continues to be a significant, and as yet unresolved, issue in establishing economic independence for women. The realization of this goal involves not only how time and responsibilities are to be allocated, but also how the physical characteristics of housing and communities influence the performance of necessary household labor. In this admirable and eloquent book, Dolores Hayden examines feminist reactions to the convulsions of nineteenth- and early twentieth-century industrialization and urbanization. In doing so she brings to light a rich legacy of innovative thought whose relevance cannot be underestimated by students of contemporary social and housing policy. In particular, this book provides a fascinating perspective for those concerned with the adaptation of our housing stock, whose physical design is frequently ill suited for the social and economic imperatives of the 1980s.

In the course of its fourteen chapters, the book discusses both the environmental and jurisdictional solutions to establishing economic equality for women, beginning with the domestic realm. The former were the most radical, for they brought an end to the isolation of the housewife and created the physical setting for the collectivization and specialization of household labor. The followers of Robert Owen and Charles Fourier had both developed strong feminist positions and believed that the private dwelling was one of the primary obstacles preventing women from improving their position in society; they further held that improved housing design was as essential to women's rights as were technological advances to the livelihood of industrial workers. While this communitarian platform for complete control of the physical environment could be implemented only within the context of a model community, the demand for jurisdictional supremacy within the household emerged in Boston in the 1860s and represents a more pragmatic approach to the organization of space within the domestic context. This movement was aided by technological economies of scale first observed in the residential hotel with its centralized cooking and laundry facilities.

Women believed that the rapidly developing improvements then occurring in the urban service infrastructure could be used so that the home could be a model of efficiency paralleling the organization of the industrial workplace. This view reached its culmination during the Columbian Exposition of 1893 with Ellen Swallow Richards's Rumford Kitchen, which fed ten thousand people. Here Hayden establishes the link between industrial efficiency harnessed by the feminist cause and the pioneering efforts of professional women to deal with the vast physical and social problems precipitated by rapid urbanization.

While technology fostered increased densities and superior urban services, both of which served to enhance the possibility for an efficient domestic environment, the forces of suburbanization spurred the domestication and miniaturization of technology in the form of the "dream house." Here, "Mrs. Consumer" had entry only through her husband, "Mr. Homeowner," for the suburbs had only the most limited access to paid employment. The social and spatial consequences of suburbia rendered obsolete the ideas generated when housing for Americans meant dense urban neighborhoods and when consumption was governed by the efficient use of resources instead of maximum demand for mass-produced commodities.

Within these events lie much intellectual ferment: how did professional women reconcile their careers with "women's place" in the home? How are the class and gender issues approached if men are excluded from domestic work and living arrangements? If coalitions are built with male reformers, what will happen when reform of the larger society takes precedence over control of woman's sphere? And if, as in the USSR in the 1920s, women are workers first and housewives second, can female labor force participation be accommodated when this was thought to be untenable in the U.S.?

This book is filled with interesting and exciting ideas that stand as a tribute to Hayden's research and to the accomplishments of visionary thinkers of the last hundred years. Their ideas should receive a warm response from those whose present agendas include the issues addressed by this book, many of which are still pending in today's built environment.

DANIEL GARR

San Jose State University

H. ROGER GRANT. *Self-Help in the 1890s Depression*. Ames: Iowa State University Press. 1983. Pp. xii, 163. \$11.95.

Contemporary events periodically inspire scholars to comb the historical record in search of a usable past. H. Roger Grant's inspirational study of *Self-Help in the 1890s Depression* is clearly cast in this presentistic mold. Grant surveys five models of fin-de-siècle self-help: community gardens, labor exchanges, cooperative stores, farmers' railroads, and utopian communities, with an eye toward illustrating the resilience of private initiative in the face of economic adversity. As he explains, "Americans are tough; it is still true that often the down-and-out can pull themselves up by their bootstraps" (p. 140).

Each of these bootstrap efforts proved short-lived, flourishing during the depression, withering as prosperity returned, only to re-emerge during subsequent crises. Thus community gardens, which

promoted self-sufficiency among the poor by enabling them to farm vacant plots of urban land, later resurfaced as the victory gardens of the Great Depression, and the "anti-inflation" gardens of the 1970s. Similarly, fin-de-siècle consumer cooperatives were both the legacy of Granger experiments of the 1870s and the progenitors of comparable ventures in the inflation-ridden 1970s. In one of the more breathtaking analogies, Grant compares the "intentional" communities of the 1890s to those founded by social and religious dissidents during and after the Vietnam War, making Edward Bellamy the ideological antecedent of the tortured twentieth-century visionary, Jim Jones.

Because he is less concerned with content than form, Grant often resorts to questionable analogies. Certainly, communitarian experiments, consumer revolts, and urban agrarianism have held a continuing fascination for Americans, providing, as they do, individualistic alternatives to private welfare and public aid. But the rise of the welfare state has fundamentally altered American society since the 1890s, a fact that Grant severely underplays. Nor does he adequately assess the impact of the welfare systems of the 1890s, or explain how bootstrap definitions of self-help differed from those of Andrew Carnegie or the Charity Organization Society. His inexplicable neglect of the voluminous literature on the COS movement leads Grant to misinterpret the aim of such charitable staples as the woodyard. Far from being "betterment programs" (p. 17) where the poor provided themselves and others with fuel, woodyards set the stage for work tests to sift the "worthy" from the "unworthy" able-bodied poor. Such indignities must have provided compelling incentives for bootstrap initiatives that might sidestep the need for charity altogether, and a more thoughtful analysis of such interaction might have revealed a great deal about citizen responses to charity, philanthropy, and economic duress. Rather than providing a solid study of the enduring qualities of American individualism, *Self-Help in the 1890s Depression* presents a series of intriguing vignettes of little-known, short-lived social experiments caught in the amber of time.

KATHLEEN D. MCCARTHY
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DAVID PAUL NORD. *Newspapers and New Politics: Midwestern Municipal Reform, 1890–1900*. (Studies in American History and Culture, number 27.) Ann Arbor: UMI Research Press. 1981. Pp. 204. \$39.95.

This slim volume centers on a statistical comparison of the number and type of local news stories and editorials found in two Chicago (*Daily News* and *Tribune*) and two St. Louis (*Post-Dispatch* and *Globe-*

Democrat) newspapers in the 1890s. David Paul Nord proposes the thesis that the "new politics" (reformism based on specific, urban-oriented issues and dependent on broad, mass political support) succeeded in Chicago because of newspaper interest and failed in St. Louis because of newspaper indifference. The new politics needed the press to mobilize public opinion because grass-roots organizations remained in the hands of the "old politics." The decision to compare these two cities stems from Lincoln Steffens's linkage in *Shame of the Cities*. Moreover, Nord believes that revisionist historians overlooked important elements in Steffens's critique of municipal reform. Thus he adopts Steffens's finding that competition for public utility contracts and franchises provided the greatest source of government corruption to serve as his barometer for judging the success of the reform movement in the two cities.

Nord draws from the work of historians such as John D. Buenker and David P. Thelen to point out the diversity of goals among the new groups calling for urban reform and how they achieved success only through forming coalitions. He borrows from the research of social scientists interested in mass communications to explain the formation of these coalitions. By the 1890s newspapers in large American cities had become a genuine mass media. The typical major newspaper could no longer survive with only a class, ethnic, or partisan audience. As urban politics in the 1890s was fragmented and decentralized, the newspaper became the agency for focusing attention on issues. As Nord points out, newspapers rarely successfully told their readers what to think, but by focusing attention on a single issue could tell them what to think about. He concludes that the new politics consisted of "agenda setting" with the goal of convincing the press to focus on an important issue.

The heart of Nord's book is a comparison of the movements to regulate public utilities, specifically street railways, in Chicago and St. Louis. In both cities citizen reform groups organized to prevent city government from giving away valuable franchises to private interests. Using content analysis of local news stories he shows that throughout the decade Chicago newspapers accepted the regulation of street railways as an important part of the "agenda" for urban reform. This press unity politicized the franchise issue and allowed for the eventual victory of the new politics. On the other hand, street railway companies often made the St. Louis newspapers house organs to defend their interests. Although the *Post-Dispatch* at times supported regulation, there was no newspaper unity on the issue and thus no real agenda setting. When St. Louis faced a franchise fight in 1898 the new politics failed because of an indifferent citizenry.

Although Nord's conclusions are hardly revolutionary, his well-written and detailed narrative account (he places his statistical findings in an appendix) of the battle over street railway regulation in the two cities does make a contribution to our understanding of urban reform politics in the 1890s.

DONALD W. CURL
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R. JEFFREY LUSTIG. *Corporate Liberalism: The Origins of Modern American Political Theory, 1890–1920*. Berkeley and Los Angeles: University of California Press. 1982. Pp. xiii. 357. \$25.00.

In *Corporate Liberalism*, R. Jeffrey Lustig distills and evaluates the new mode of thinking that emerged during the Progressive era to define the political culture of industrial America. One must go back to the Revolution to find a parallel to the richness and diversity of the political thought of this period. The fundamental challenge posed by the business corporation to the liberal assumptions of the nineteenth century lay at the center of this intellectual ferment, and the reconstruction of liberal theory that followed this challenge provides the critical focus of Lustig's book.

The analysis is wide ranging and loosely knit. The author is not interested in a critique of the work of this or that Progressive theorist. Instead, he groups a number of thinkers together in each chapter and allows the argument to move rather freely among individuals, movements, and actual reforms. This is an effort to glean the "cultural logic" of Progressivism as we have received it from the bits and pieces in which it appeared, and, as such, it is not likely to satisfy those sensitive to the finer distinctions. At best, the author displays an informed disregard of the problems of employing individual thinkers in the service of synthetic interpretation.

The synthesis itself, however, commands serious attention on its own terms. It is pointed, provocative, and central to the issues of our day. The first part of the book argues that the rise of the corporate economy fractured the assumptions of early American liberalism and placed the nation at a cultural crossroads. By 1900 the choice had been made to follow the path blazed by the late nineteenth-century corporatists. Rather than attempting to adjust the new economy to fit our traditional political goals, we chose to adjust our politics to accommodate and support the realities of corporate power. The second part of the book juxtaposes the Progressive's zeal for discarding the old liberalism with the difficulties they had in the requisite task of theoretical reconstruction. It argues that this task was delimited by an acceptance of corporate power and organization and that this acceptance yielded a political

theory that was incomplete, shallow, and tragically flawed.

Lustig is certainly not the first to focus on the Progressive's attitudes toward corporate power. But where most find ambivalence—a mixture of suspicion and expectation—Lustig finds theoretical bankruptcy. He tests the Progressive's enthusiasm for political spontaneity and pragmatic institutional adaptation, looking for a meaningful set of political goals and public purposes. Each of the building blocks of the new liberalism—its theory of groups, its theory of knowledge, its theory of the state, its theory of authority—is reviewed only to reveal a justification for new concentrations of power without adequate provision for a vital political community. The promise of American life is reduced to rising levels of consumption, and political obligation is rooted in a harsh but elusive discipline of future rewards.

Lustig will not let our current liberal critics escape his critique of the Progressives. In a telling commentary he argues that even as these critics expose the crisis of public authority inherent in Progressive reform they continue the Progressive counsel of adaptation in politics and government. Like the Progressives, they have failed to come to terms with the question that lies at the heart of the crisis; that is, the legitimacy of the business corporation in a liberal polity.

STEPHEN SKOWRONEK
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Los Angeles

H. LEON PRATHER, SR. *Resurgent Politics and Educational Progressivism in the New South: North Carolina, 1890–1913*. East Brunswick, N.J.: Fairleigh Dickinson University Press. 1979. Pp. 303. \$24.50.

H. Leon Prather, Sr., has provided a straightforward, useful monograph that ties together important political and educational developments in North Carolina during the Populist and Progressive eras. Although his work partially overlaps that done earlier done by Helen Edmonds, Louis Harlan, Oliver Orr, C. Vann Woodward, and others, Prather provides more detailed treatment with particular emphasis on the role of Tarheel blacks, both as actors and victims.

The major purposes of the study are, he explains in the preface, "to describe how a unique mixture of politics and racial attitudes coalesced to involve education, and to identify and analyze the major forces associated with and propelling the public school movement between 1902 and 1913" (p. 11). Accordingly, after describing the woeful condition of public education in North Carolina in the late nineteenth century, Prather recounts the political

battles of the 1890s, which resulted in the temporary ascendancy of the allied forces of the Populists and Republicans, the "Fusionists." Conceding that a conservative state supreme court and other obstacles largely blocked Fusionist efforts for educational reform, Prather nevertheless asserts that "the Populists became the first to deal constructively with the public education problem" and enacted "the most progressive legislation since Reconstruction" (p. 124).

When the Democrats regained control by playing what Prather aptly terms the trump card of white supremacy in the fiercely racist campaigns of 1898 and 1900, "the white man's party" both disfranchised most Negro voters (through a literacy test with a grandfather-clause loophole for illiterate whites) and, led by Governor Charles B. Aycock, proceeded to put North Carolina in the vanguard of the South's belated educational awakening. Black children and their schools, in North Carolina as elsewhere in the South, were the victims of a separate but vastly unequal educational system, and Prather describes the inequities in a short chapter toward the end of his study.

ROBERT F. DURDEN
Duke University

JOHN PATRICK MCDOWELL. *The Social Gospel in the South: The Woman's Home Mission Movement in the Methodist Episcopal Church, South, 1886-1939*. Baton Rouge: Louisiana State University Press. 1982. Pp. x, 167.

John Patrick McDowell provides considerable documentation for his contention that the Social Gospel, so often discussed with principal focus on the North, was adopted by the leaders of the woman's home mission movement in the Southern Methodist church. They read, digested, and recommended the books of the recognized leaders of Social Christianity: Washington Gladden, Josiah Strong, Walter Rauschenbusch, Shailer Mathews, Charles Stelzle, Richard Ely. As did Social Gospel advocates in other denominations and sections, they committed themselves to "building the Kingdom of God on earth."

The first chapter describes the organizational aspects and guiding visions of Southern Methodist woman's home missions from 1886 to Methodist unification in 1939. The subtitles of the other four chapters list the groups that were the object of the missions' efforts in "Extending the Kingdom": "At Home and at Work," "To Immigrants and Seeking Peace," "To Blacks," and "To Women." Like other Social Gospel movements, this one aimed at the Christianization of America and at resisting "threats" posed by immigrants. Only since World War I did negative attitudes toward foreigners

begin to change and the sense of superiority toward blacks begin slowly and unevenly to moderate, influenced by closer encounters with black women and justified by reference to biblical passages and to environmentalist and evolutionary concepts. The leaders of the movement were usually comfortably situated, "leisured" persons who only gradually moved from a patronizing condescension toward those whom they sought to "uplift"—a key word that frequently appears in the book.

Basing his account largely on annual reports, periodical articles, and books by and about Southern Methodist women, McDowell has done his work competently and illustrated it with apt quotations. He emphasizes their positive contributions but notes places where practice varied from profession, as in race relations and in neglect of the plight of tenant farmers. He also finds that little was said about labor unions with no support offered for militancy and concludes that in this they were moderates like the Gladden of *Applied Christianity*. Yet that book contains one of Gladden's more forceful arguments on the right of labor to organize and to fight for its rights if other approaches fail, for sometimes war is the lesser of evils. McDowell's book thus does not properly emphasize a major difference between the Social Gospel's mainstream and this regional adaptation at the point of attention to unions. One wonders also why there is not more attention to the interdenominational Council of Women for Home Missions (1908-40) of which this Southern Methodist organization was a member, but which is mentioned only in passing. In view of the book's main title, at least some comparison with the social witness of other Southern home mission agencies—Methodist, Baptist, Presbyterian—would have been appropriate.

Given their situation and context, the women at the center of Southern Methodist home missions made courageous and effective contributions to Christian social thought and action in the South and played a part in the struggle for a larger place for women in church and society. Their story is generally well told in this useful study, completed by a good bibliography and index.

ROBERT T. HANDY
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RUTH ROSEN. *The Lost Sisterhood: Prostitution in America, 1900-1918*. Baltimore: Johns Hopkins University Press. 1982. Pp. xvii, 245. \$18.50.

The movement to eradicate the open prostitution that existed in most American cities during the Progressive era was one of the major reforms of the period, and *The Lost Sisterhood* comprises a fresh and

important approach to this revealing episode in our history. Ruth Rosen examines prostitution as both a "cultural symbol" and a "social institution that women used as a means of survival" (p. xiii), and, after introductory chapters tracing the changing views of prostitution in the late nineteenth century, and outlining the general approach to prostitution in the Progressive era repression, she concentrates on exploring the relationship between the world of the antiprostitution reformers and the world of the prostitutes themselves.

Rosen shows that the antiprostitution movement sometimes viewed the prostitute as a victimized innocent girl, and at other times as a "vicious woman," a conflict that reflected the tug of war going on in the Progressive years between an older, traditional moralism and the new scientific environmentalism, as well as a confusion over female sexuality. Rosen also discusses the role of organized women's groups in the antiprostitution movement, which is enlightening, even though most of the positions taken by women were also advanced by men. Rosen's general argument is that most of the men and women in the antiprostitution movement, because of class, cultural, or gender biases, did not fully comprehend the effects of their efforts on the women they were trying to help. The most important distinction between Rosen's work and other work on the subject, which is implicit in her title, is the obvious time and effort she has devoted to understanding the lives of the women who became prostitutes in the Progressive era and her humane effort at communicating the texture and historical significance of these lives to the reader. (Here Rosen builds on her earlier work as historical editor of *The Maimie Papers* [1977], a collection of the letters of an occasional prostitute in the Progressive years.) A central theme of *The Lost Sisterhood* is that while physicians saw prostitution as a source of venereal disease, moralists as a sign of the breakdown of traditional morality, nativists as a result of unrestricted immigration, socialists as a form of economic exploitation, and feminists as a means of the social control of all women. Prostitutes saw prostitution not as a symbol, symptom, cause, result, or abstraction, but as a means of survival in the raw and unforgiving urban industrial economy. Rosen argues that for the working prostitute, the vice district, and its main institution, the brothel, constituted a single-sex subculture, with its own customs (such as taking a new name when entering prostitution), history, language, and humor, which, Rosen maintains, provided the women with protection, support, and a sense of human worth. Rosen makes use of numerous memoirs of prostitutes (which she treats with the skepticism that these documents of dubious origin warrant), as well as other contemporary material on the vice districts. She carefully avoids any

romanticization of the prostitutes' lives, however, and finally sees prostitution as a depressing manifestation of the lack of choices afforded women during these years.

The Lost Sisterhood, which is illustrated with many of E. J. Bellocq's famous and melancholy photographs of the Storyville red light district in New Orleans, adds an important dimension to our understanding of the somber complexities of the Progressive years, and, just as importantly, to our understanding of the constraints gratuitously imposed on poor and working women during this much-ballyhooed period of our history.

MARK T. CONNELLY
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MANSEL G. BLACKFORD. *A Portrait Cast in Steel: Buckeye International and Columbus, Ohio, 1881-1980*. (Contributions in Economics and Economic History, number 49.) Westport, Conn.: Greenwood Press. 1982. Pp. xviii, 225. \$29.95.

Until 1980 Buckeye International was a medium-sized industrial firm located in Columbus, Ohio, a medium range American city. According to Mansel G. Blackford of Ohio State University, it is these circumstances that provide his work with a general significance. He suggests that the mutually beneficial ties connecting Buckeye and Columbus may be characteristic of similarly sized businesses and communities elsewhere, and indeed that local networks are of critical importance for the success of small business operation in middle America. Despite the rise of a national economy, much of American business activity is apparently still dependent on "island communities."

Beginning as a malleable ironworks in the 1880s, Buckeye clung tenuously to life for almost a decade until it concentrated on producing iron and then steel couplers for the railroad industry. Now operating in a concentrated market with few rivals and little price competition, the company carved out a small but secure niche for itself until the railroad industry declined precipitously after World War II. This initial success is largely credited by the author to an aggressive and innovative upper management. Coming from backgrounds in engineering and the railroad industry, upper management rose from within the company to make Buckeye a life-long career, and in the process ended up as major stockholders in a business that very much resembled a private club.

Aware that Columbus's well-being was important to the company and its workforce, and finding it an attractive place to live, management was involved in a variety of civic activities. In all Blackford finds

management motivated by a view of the harmony of interests between business, employees, and community. Unfortunately, the material he has had to work with in regard to personnel policies and the company's involvement in Columbus is insufficient to develop and support adequately this line of reasoning, the major weakness in an otherwise excellent and thorough work.

The author does demonstrate impressively the importance to Buckeye of a local network that could reach out when necessary to the banks of Cleveland and the national railroad industry and on which the company drew for directors, managers, sales contracts, and most importantly capital for modernization, expansion, and survival in hard times. It is the existence of this personal network rooted in community that the author proposes as the key to the survival and success of much of small business.

Tied to the railroad industry, Buckeye by the late 1950s was in serious trouble, and run by a conservative and parochial management. To survive, Buckeye needed to diversify and grow. A new breed of manager was brought in from the outside who succeeded in making the company sufficiently attractive that by the end of the 1970s it was the object of hostile takeover attempts. Finally in 1980 Buckeye was merged with Worthington Industries. The fact that the latter was also a Columbus company facilitated a friendly acquisition.

For his history of the company until World War II, Blackford has relied mainly on company records that now have been deposited at the Ohio Historical Society. For the period since 1960 he has interviewed many of those involved in management. Interestingly these interviews allowed for much greater insight into decision making than the records. It is unfortunate that the author did not also interview rank-and-file workers or politicians and others involved in city government. This is a good business history in terms of the treatment of upper management and the company's place in the steel industry, but when it seeks to relate Buckeye to the growth and governing of the community, sufficient detail is lacking for any meaningful analysis.

STANLEY BUDER
Baruch College

RICHARD C. OVERTON. *Perkins/Budd: Railway Statesmen of the Burlington*. (Contributions in Economics and Economic History, number 45.) Westport, Conn.: Greenwood Press. 1982. Pp. xxiv, 271. \$29.95.

This volume is a fascinating business biography of two railroad executives, Charles E. Perkins and Ralph Budd, during their respective years as president of the Chicago, Burlington & Quincy Railroad. The CB&Q started out as a short branch line near

Chicago in 1850 and a short century later had become a system of more than 8,700 miles. The Burlington during its corporate history has been known for its financial responsibility, has never been near receivership, and has paid dividends without a break from 1863 until 1970, when it was merged with the Great Northern and the Northern Pacific into the Burlington Northern. Perkins was president of the Burlington from 1881 to 1901, while Budd was head of the railroad from 1932 to 1949. Both men were honest executives in sharp contrast to the robber baron stereotype of an earlier era. While a long generation separated the different environments during which the two men served, their points of view were frequently quite comparable. Richard C. Overton lets the two men speak for themselves, using transcripts of thirty-five memoranda written by Perkins between 1875 and 1907 and some sixty talks delivered by Budd between 1923 and 1949.

The Cincinnati-born Charles Perkins (1840–1907) was early orphaned, and in 1859 was offered a clerkship at thirty dollars a month on the Burlington & Missouri River Railroad by his cousin John Murray Forbes, a Boston financier with a large investment in the Burlington. Perkins was given his chance by his cousin, but his rapid rise—general superintendent in 1865, company director in 1875, and president of the CB&Q in 1881—was due to merit and ability. In the memoranda sent out to subordinates from his Burlington, Iowa, headquarters, Perkins's strong views on the law of supply and demand and laissez-faire economics were put forth with vigor. He believed that wages could only be set by the "solid bedrock of supply and demand" (p. 6), that pensions demoralized employees, and that railroad managers had no right to spend corporate money for charitable purposes. As a firm believer in pooling and freight rate stability, Perkins was an early critic of the 1887 Interstate Commerce Act that prohibited the pooling of traffic. In 1901, the year of his retirement from the presidency, Perkins strongly supported the sale of the vast majority of Burlington stock to James J. Hill's northern lines, the Great Northern and the Northern Pacific.

Ralph Budd (1879–1962) was raised in Iowa and as a young, college-trained civil engineer worked for a decade on several railroads before moving in 1909 to an Oregon railroad owned by James J. Hill. In 1912 Hill invited Budd to be assistant to the president of the Great Northern. At the age of forty, in 1919, Budd became the president of the Great Northern, a position he held until moving on to the presidency of the Burlington in 1932. During his half-century of railroad service Budd proved to be a modest and knowledgeable expert with a keen sense of history. The dozens of talks given to fellow

railroaders, businessmen, and the general public were all marked by logic, clarity, and credibility. Unlike Perkins, whose presidency came during years of railway expansion, Budd's tenure included new competition for railroads, the Great Depression, and World War II. Like Perkins, Budd saw little need for greater railroad regulation by the government. He believed that greater rail merger activity would help the industry give better service to the public. Budd was also an innovative president. He introduced lightweight, diesel-powered passenger trains, the first of which was the *Pioneer Zephyr* in 1934, and the vista-dome passenger car at the end of World War II.

This scholarly volume brings the same high excellence to railroad history that was true of Overton's earlier books on the Burlington. Included in the text are several benchmarks that list price indexes and mileage statistics for the decades before and during the tenures of the two presidents. The author has also included a brief interlude for the years between 1901 and 1923, as well as an epilogue that reviews Burlington highlights of the last three decades. In writing *Perkins/Budd* Richard Overton has produced a revealing study of the railroad executive mind at work that will be welcomed by all students of transportation and business history.

JOHN F. STOVER
Purdue University

WILLIAM G. ROBBINS. *Lumberjacks and Legislators: Political Economy of the U.S. Lumber Industry, 1890-1941*. (Environmental History Series, number 5.) College Station: Texas A&M University Press. 1982. Pp. xiv, 268. \$22.50.

As the fifth volume in the publisher's useful "Environmental History Series," William G. Robbins's book chronicles the ambivalent role of the lumber industry during years of important alterations in the American political economy. He supports the "search for order" interpretation of the period. But in several important respects, this history is not substantial enough to fit into that thesis.

The title itself is deceptive. Robbins does not deal with lumberjacks, if that word implies the labor force that was indeed a basic problem for the industry. Nor does he assess the work of lawmakers—even the long record of the most indefatigable of them during those years: Oregon Senator Charles McNary. There is no analysis of timber-state legislation—especially Oregon's comprehensive forest management laws early and late in those decades. It also seems strange that the author did not use the archives of any lumber companies. Some of the finest such collections—Weyerhaeuser's near Seattle, several others in that city and at the Univer-

sity of Washington, and the George Jewett Papers at the University of Idaho—are within a day's driving distance of his desk.

The book does survey the spectrum of lumbermen's demands for federal support and hostility to reasonable government regulation of their industry and shows the inertia that their attitudes produced not only in statutory solutions but also in intra-industry practices. Two of the main figures in Robbins's story were essentially cheerleaders without a team or a crowd. Wilson Compton, executive of the National Lumber Manufacturers Association, and consulting forester David Mason, each devoted nearly forty years of their lives to educating lumbermen and acting as liaison with federal forest agencies and Congress. But the author does not note—except in one late-dated Jewett letter—that lumbermen did not respect or trust the former because he was an economist by training and an Easterner. He also fails to assess Mason's heart-breaking tenure as manager of the Lumber Code Authority under the New Deal's National Recovery Administration. That assay in industrial self-government is not clearly or adequately covered by this volume, though it is the central episode of the author's theme.

This reviewer is unable to account for why a well-regarded scholar and forest historian was satisfied with this history. Perhaps he stopped working too soon. A chapter or two should have examined the role of lumbermen and legislators in writing the two most promising efforts made to reconcile the public interest and capitalism: the cooperative forestry statutes of 1937 and 1944, each briefly mentioned in this volume. Perhaps the search for effective regulation of the lumber economy is too complex to be presented by any one historian within the limits of a single volume.

ELMO RICHARDSON
Seattle, Washington

ALEXANDER CAMPBELL MCGREGOR. *Counting Sheep: From Open Range to Agribusiness on the Columbia Plateau*. Seattle: University of Washington Press. 1982. Pp. xii, 483. \$25.00.

Business history, family history, and local history are often narrow and tedious. Alexander Campbell McGregor, artfully weaves them all together to make the story of the McGregor Land and Livestock Company an attractive and illuminating case study of change in American agriculture since the late nineteenth century. Four enterprising Scottish brothers came from Canada to semiarid, rough, and remote eastern Washington and aggressively exploited frontier opportunities. They rode the region's escalator of economic growth. Transhumant sheep raising led to land acquisition. Raising wheat

hay for winter feed led to commercial grain growing. Supplying employees led to a mercantile operation. One venture parlayed on another, and the whole enterprise eventually involved hog and cattle feeding, farm implement sales and manufacture, an auto agency, real estate and farm commodity brokerage, insurance, a company town, irrigation works, fruit raising and packing, and fertilizer distribution.

From the start the McGregors' aim, like that of most farmers, was to make money. Their partnership was converted into a family corporation in 1905, an early example of corporate agribusiness that grew in size and scope while spinning off subsidiaries. Twentieth-century changes in agriculture that favored an enlarged scale of operations were reflected in the McGregor enterprises. McGregor successes were built on free grazing of the public domain, cheap land, unearned increment, tariff protection, easy federal credit, government price supports, and public research in agricultural science and technology. To that the McGregors added their own thrift, shrewd dealings, luck, the employment of expert help, and readiness to adopt technological innovations replacing labor with capital. The story is not merely an affectionate one of continuous success and family tranquility. The author does not minimize discord, bad judgment, and hard times.

Unusually full business and family records provide the source material of this study. That was supplemented by interviews, railroad and court records, and a wide array of secondary works to broaden the book as a revealing chapter in twentieth-century agriculture.

JAMES H. SHIDELER
University of California,
Davis

OLIVIER ZUNZ. *The Changing Face of Inequality: Urbanization, Industrial Development, and Immigrants in Detroit, 1880-1920*. Chicago: University of Chicago Press. 1982. Pp. xix, 481. \$43.00.

The roster of American cities whose development can be viewed in social structural terms has grown rapidly in recent years. With the publication of *The Changing Face of Inequality*, Detroit now joins that list, and it is a welcome addition indeed. Based on an innovative method of generating citywide data through selective block analysis, Olivier Zunz provides us with an array of new information with which to examine Detroit's spatial, demographic, ethnic, and socioeconomic structure from 1880 to 1920, as well as patterns of housing construction, homeownership, residential persistence, and the distribution of city services.

To Zunz, as to other scholars who have examined narrower slices of Detroit society, ethnicity is the key to unlocking the city's late nineteenth-century social order. To understand life there is to appreciate the diversity and autonomy of the ethnic ministates that taken together comprised the city and that taken separately guided the values and behavior of their members at home, at work, at the polls, and at the altar. But, with ever accelerating force the leaders of one of the ethnic groups—the white, native-born Protestants—armed with wealth, technology, and the ideology of modernization, destroyed the ethnic system and replaced it with a new social order headquartered in the factories and boardrooms of post-1900 Detroit. Departing from those scholars who, like Tamara Hareven, argue that the ethnic groups survived the challenge of the factory order, Zunz is convinced that, at least in Detroit, the victory of the new order was swift and overwhelming.

Attractive in method and rich in information and insight, *The Changing Face of Inequality* is nonetheless open to challenge in interpretation. Zunz is clearly attracted to the idea of autonomous ethnicity, but he never establishes how much autonomy individuals or families living in the "old regime" actually possessed. He argues, for example, that the new factory system successfully isolated workers both from their ethnic communities and from one another, but he presents no evidence to suggest that workers living in the new blue-collar suburbs had less influence over their community lives than their working-class parents did when living in ethnic compounds dominated by ethnic elites of one sort or another. Neither does his view explain how the owners' success in keeping their workforce docile and fragmented in 1920 evaporated so quickly in the years thereafter. It may well be that Zunz has overstated both the liberating qualities of the old system and the repressive side of the new.

Like many social historians, Zunz is more forthcoming in revealing his methodological assumptions than his value preferences. One cannot fault him for preferring ethnic to factory culture, but this excellent book bears the impact of the author's unstated values on the treatment of deceptively neutral information.

RALPH JANIS
Cornell University

DINO CINEL. *From Italy to San Francisco: The Immigrant Experience*. Stanford: Stanford University Press. 1982. Pp. viii, 347. \$25.00.

Dino Cinel makes an important contribution to the recent boom in scholarship on the people of San Francisco. His thesis is that the Italian immigrant

experience there was one of change within continuity. This summary, however, fails to convey his deep and careful exploration of nuance and counterpoint in that experience. He is sensitive to crucial but too often ignored variables in immigration history such as time of arrival, immigrant intentions to repatriate, and differences in homeland situation and experience prior to emigration.

Cinel has developed to new levels the art of linking specific immigrants to their Italian origins. The heart of his research is a sample of emigrants from nine Italian communes. He worked backward from San Francisco to identify them in their individual families and home villages, and tracked their children as they came of age in San Francisco. His analysis, then, is built from data from three generations: the immigrants, their parents, and their children. With this evidence he is able to expose and explain variations in the immigrant experience within the San Francisco community. For example, he explores a variety of potential Old and New World influences in probing the reasons why immigrant fishermen from one village followed their traditional trade in California while those from another town in the same region in Italy abandoned fishing much more often.

Cinel recognizes the unusual features that San Francisco presented to the immigrants and is prudent in his generalizations about Italians elsewhere in America. Nevertheless, his book is much more than a history of Italians in a single American city. His analysis of immigrant origins will be of great value for all immigration scholars and is a contribution to Italian social history.

It seems always the case that there are things that a reviewer would have liked to have an author do differently. There is no exception here. Choice of a satisfactory occupational classification scheme is a persistent problem in social history. Cinel adopted the U.S. census pattern, which stresses functional and industrial sector similarities over socioeconomic status characteristics. Although his choice yields some advantages in studying San Francisco Italians, I am not convinced it represents an overall improvement on the more common status-sensitive approach. He keeps his presentation of statistical data to a minimum in the text. While generally this is commendable, he is too parsimonious at times, for example, I would have liked for him to have indicated the number of cases in the cells of his tables. He only reports percentages. Finally, his conclusion, perhaps necessarily, does not capture the richness and complexity of his analytical chapters.

My criticisms, however, are insignificant in light of the achievements in this book. Cinel has set a high standard for immigration studies.

JOHN W. BRIGGS
Syracuse University

PETER R. SHERGOLD. *Working-Class Life: The "American Standard" in Comparative Perspective, 1899–1913*. Pittsburgh: University of Pittsburgh Press. 1982. Pp. xvii, 306. \$21.95.

JOHN BODNAR. *et al. Lives of Their Own: Blacks, Italians, and Poles in Pittsburgh, 1900–1960*. (Working Class in American History.) Urbana: University of Illinois Press. 1982. Pp. 286. \$22.95.

These two carefully crafted complementary monographs in comparative labor and ethnic history reflect the farthest limits in methodological finesse and sophistication to be thus far attained by social and economic historians studying the lives of American workmen. In both works the setting is Pittsburgh primarily beginning in 1900 when the larger Pittsburgh region produced 64 percent of the country's structural steel, 57 percent of its crucible steel, nearly 57 percent of its Bessemer steel, and 50 percent of its coking coal.

From Carnegie and Gompers down to William Z. Foster and Thomas C. Cochran, knowledgeable Americans have invariably asserted that American workmen have earned higher wages and enjoyed a higher standard of living than workmen anywhere else in the world. *Working-Class Life* is a painstaking comparative study that should lay that controversy to rest. To put the "American standard" to the test, Peter R. Shergold, a student of the pre-eminent historian of Anglo-American labor, Charlotte Erickson, meticulously researched the everyday lives of British and American workmen in the twin Anglo-American iron and steel cities of the North Atlantic. Deliberately selecting the time frame, 1899–1913, when business cycle movements were marked by a high degree of correlation, Shergold rigorously analyzes comparative data for Birmingham (supplemented by Sheffield) and Pittsburgh. In ten chapters of straightforward prose augmented by eighty tables, charts, and graphs, he marshals evidence drawn from a wide array of often ingeniously selected conventional and statistical sources. Shergold details comparative wage rates, family incomes, food prices (inadvertently, alcoholic beverages are omitted although one-quarter to one-third of the workmen's food budget is said to have been so expended), rents, shopping options, and the costs of fuel, clothing, and furniture. Based undoubtedly on what must have become the most learned comparative consumers' handbook ever compiled for two industrial cities, the author concludes, not surprisingly, that skilled Pittsburgh workmen earned far more than their Birmingham counterparts and were indeed the world's labor aristocrats. On the other hand, unskilled workmen in both cities, native-born in England, largely foreign-born in the United States, received equal wages. In the United

States, however, the disparity between the skilled and the unskilled and the skewed distribution of wages in "the greatest hive of human industry on the face of the earth" (p. 15) was exaggerated by ethnic divisions unknown to the homogeneous Birmingham working population that was forging a new labor solidarity. The new immigrants did not share the consumption ethic of native-born Americans and established older immigrants or of British workmen. They had other goals. They looked back to their preindustrial homelands, and they showed a propensity to save, to bank, and to remit much of their incomes. Total strangers to urban industrial America, most of them, including blacks, planned to return home, an impulse that was intensified by nativist sentiment. In years of mass unemployment such as the depression year 1907–08, not surprisingly, Allegheny County contributed a greater number of returnees than any comparable area in the country. The widely entertained assumption that the comparative affluence of the average American workmen dashed hopes of all socialist utopias, "on the reefs of roast beef and apple pie" (p. 9), in Werner Sombart's pungent oft-quoted phrase, made allowance neither for disparate ethnic diets and folkways nor for the ethnic and wage stratification that divided Pittsburgh and much of laboring urban-industrial America into what appeared to be two nations. Regarded as less than men by native and earlier immigrant stock who could neither understand the newcomers nor appreciate their aspirations, "they can't talk United States" (p. 52) summed up more than a language dilemma, even for those who could speak English and who were accordingly paid two cents extra per hour. In 1900, 70 percent of Pittsburgh's major immigrant groups still consisted of Germans, Irish, and English, and that proportion among the city's ethnics would have been even higher if the second generation was included.

In *Lives of Our Own*, the three collaborating historians do not portray Pittsburgh in all of its ethnic complexity but for the first time bring into full view the interplay among the major preindustrial peoples—blacks, Italians, and Poles—who were concentrated predominantly in the working class, who migrated at about the same time, and who persisted for three generations. Building on the rich vein of comparative and multiethnic historical scholarship associated with the work of Josef Barton, Tamara Hareven, Thomas Kessner, Stephen Thernstrom, and others, John Bodnar, Roger Simon, and Michael P. Weber go even further. They give voices to the anonymous, speak from their perspectives, and focus on their problems as they saw them and on the goals that they set for themselves, as recorded in extensive oral interviews. Recognizing and identifying subethnic as well as ethnic groups and eschew-

ing aggregate statistics, they have traced and analyzed differential and often contrasting migration, employment, family, neighborhood, and housing patterns. What emerged, they conclude, was not a unified working class but "a segmented mass with deep fissures running across occupational, neighborhood, racial, and cultural lines" (p. 264) that found blacks at a disadvantage, the victims of long-term forces and pressures that cannot simply be attributed to rising racial antipathies. Without stable occupational and community support, blacks came to Pittsburgh in greatest numbers between 1930 and 1960 when a slowing urban economy, solidly entrenched second-generation ethnic, occupational, residential, and family systems, and the recentness of their migration left them isolated. Without a base, they continued to remain urban first generation in status.

These two books not only enlarge our understanding but also should open the way to a long-overdue full-scale assessment of the complex human networks that peopled and energized the industrial American heartland whose future now appears so problematic.

MOSES RISCHIN

San Francisco State University

JAMES W. BYRKIT. *Forging the Copper Collar: Arizona's Labor-Management War of 1901–1921*. Tucson: University of Arizona Press. 1982. Pp. xiv, 435. \$24.95.

On July 12, 1917, the copper mining town of Bisbee, Arizona, lived the most dramatic day of its history as some 1,186 striking miners and supporters were dragged from their homes, shoved on a special train, and deported to a New Mexico location 170 miles away. Behind this paramilitary action perpetrated by local citizens' committees and vigilante groups stood the whole establishment of the community—from the county sheriff to the local Bell Telephone manager. The careful planning and execution of this mass deportation prevented the kind of tragic consequences that one associates with labor crises such as the Homestead strike or the Ludlow massacre. Yet, as James W. Byrkit makes clear, the Bisbee *affaire* stands as one of the darkest chapters in American labor history.

Forging the Copper Collar presents the most thorough reconstruction of that event and is likely to become the standard work on the subject. Based on solid archival research, the narration is so skillful that at times it reads more like a thriller than a historical monograph. The author's main concern is to show that what happened on that day was not an isolated episode. If his book takes us from the early days of the Arizona copper mining industry to the aftermath of World War I, it is because the Bisbee

deportation stands as the turning point in a short but intense history of labor-capital confrontation. In Arizona this confrontation had become particularly acute because of the vitality of that state's Progressive movement and the leadership role held by organized labor; but also because of the political cohesion existing among the copper barons and of their determination to become sole masters of the industrial and political life of the state.

The corporations engineered their antilabor offensive very skillfully, mobilizing the local press, the schools, and the pulpits. But it was the American entry into the war and the presence of the Industrial Workers of the World in the state's mining industry that provided the crucial ingredients that made the deportation possible. Byrkit, in fact, does an excellent job showing how the copper magnates seized on the growing wartime hysteria to brand any striker—Wobbly or not—as a saboteur and traitor. When news of the deportation reached the outside world, the action was welcomed overwhelmingly as a courageous act of patriotism. The timid reaction on the part of federal authorities did the rest. The stage had been set for a countrywide crusade that would culminate, a few months later, in the massive national IWW arrests and trials and the subsequent Palmer raids.

Byrkit goes a long way trying to *demystify* the Wobblies' image as agents of subversion or even as effective labor militants. In the industrial scenario that he has depicted, the Wobblies are more important for the way they were used by the antilabor forces than for their alleged revolutionary practice. It is clearly a challenge that IWW historians will have to take seriously. Beyond this, however, the reader will find very little that is not already known from the existing literature on that organization.

Another source of disappointment, perhaps more conspicuous than the previous one, is the author's failure to provide a more rigorous analysis of Bisbee's middle class. Throughout the book one reads that this sector of the community had a determining role in the shift of support away from labor and in favor of the copper corporations. But in the end, the middle class figures more as a vague background piece than as the key protagonist the author contends it was.

Despite these weaknesses, *Forging the Copper Collar* is an excellent example of how economic, political, and labor history can be integrated to advance our knowledge of one of the most important chapters in American local and national history.

BRUNO RAMIREZ
University of Montreal

ARTHUR LINK, *et al.*, editors. *The Papers of Woodrow Wilson*. Volume 36, *January 27–May 8, 1916*. Prince-

ton: Princeton University Press. 1981. Pp. xxiv, 684. \$30.00.

ARTHUR LINK *et al.*, editors. *The Papers of Woodrow Wilson*. Volume 37, *May 9–August 7, 1916*. Princeton: Princeton University Press. 1981. Pp. xxiv, 566. \$30.00.

ARTHUR LINK *et al.*, editors. *The Papers of Woodrow Wilson*. Volume 38, *August 7–November 19, 1916*. Princeton: Princeton University Press. 1982. Pp. xxvi, 716. \$30.00.

The monumental project to publish the papers of Woodrow Wilson, undertaken a generation ago by the Woodrow Wilson Foundation and the Princeton University Press, is now at a crucial point, virtually the end of the calendar year 1916, the beginning of American involvement in war and peace, and it is a proper occasion to examine the entire enterprise. Volumes 36, 37, and 38 are all for 1916, from January 27 through November 19. Ensuing volumes will tax the editorial staff because of wartime complexities; then will come the almost equally complicated peace conference; and with a volume or two on the administration's final months and the president's last years at the "S" Street house in Washington, the grand project will be complete.

The Papers of Woodrow Wilson was of course not the first of the presently ongoing projects to publish papers of major figures in American history, but it surely has proved one of the most successful—along with the Jefferson project, likewise at Princeton, the Franklin project at Yale, and the Adams project at the Massachusetts Historical Society. Other papers projects for a while appeared desirable, for college enrollments were going up and up, academicians were filled with an enthusiasm that partook of increasing salaries, and quite possibly some ambitious individuals disliked teaching or found research awkward or enjoyed soliciting funds from foundations and the federal government. When the pinch came, many of the papers projects, for figures of secondary historical importance, appeared to have been hardly necessary. But projects for major figures have continued, and have been well handled, and none better than *The Papers of Woodrow Wilson*.

Reasons for the success of the Wilson project are several. One is an editorial board of distinction—presently Henry Steele Commager (emeritus), Katherine E. Brand, John Milton Cooper, Jr., William H. Harbaugh, August Heckscher, Richard W. Leopold, and Arthur M. Schlesinger, Jr. Another is the guiding hand of Arthur S. Link, not merely an editor but the acknowledged master of Wilson scholarship, author of a dozen books mostly on Wilson. His assistants likewise are highly capable—David W. Hirst, John E. Little, Frederick Aandahl, Phyllis Marchand, Margaret D. Link. In a single

decade, 1973–82, twenty-six volumes and two cumulative indexes appeared. The Wilson factory occupies a small suite in a corner of the Firestone Library, down a labyrinthine corridor designed, one thinks, so that merely curious visitors get lost. There the staff comes in early each day and leaves late, and everyone works to turn out the books that are, physically, very appealing, handsomely designed and bound, nothing about them formal or celebratory but clearly for use, with colors of brown and gold, a nice brownish paper, each volume graced by a circled photograph of the president on dust jacket and title page. Volumes are set by linotype by two typographers who have worked with the project for years—hence there are no tapes by anonymous typists, done perhaps in their apartments where they can lose pages of manuscript and spill coffee on the tapes. Editors read galleys three times, page proofs three times, corrected pages twice; the indexer reads pages word for word. “Our rule in the Wilson Papers,” writes Editor Link, with due pride, “is that our tolerance of error is zero.” Everything thus is done in the manner of books before the age of editorial assistants. And not merely does the Princeton University Press use linotypes but it prints in signatures that are stitched in the old-fashioned way and glued to cloth—no so-called perfect bindings here.

The Wilson papers are a selection. And there was need to print other materials including letters to Wilson, so as to give a story not always in Wilson’s viewpoint. The editors have made a wide sweep of materials by and about Wilson, records not merely of the State Department in Washington but of foreign offices abroad; of other United States government departments; state archives; records of labor unions; museums; private papers in university libraries or archives, private libraries or archives, or in private possession (volume 38 includes letters, just discovered, from Wilson to Jessie Woodrow Wilson Sayre).

The three volumes under review have photographic sections, and the photographs are mostly new, and in all cases sharp, not the banal and blurry photographs that many authors and editors place in their books almost as an afterthought. Captions sometimes show humor, as in that for the Speaker of the House in 1916, who wears a large beaver hat; the caption says, “The Speaker of the House under cover.”

Each volume opens with a rundown of events, followed (after a statement of editorial procedure) by an abbreviated index of contents so the reader can look up something quickly. Indexes at the ends of volumes are masterful—volume 36 has twenty-four pages of double-column entries in small point.

Fortunate it was that the project got started when it did, in the 1950s when aging Wilsonians were

donating their papers to depositories and the editor could find out where they were going. Then, equally fortunate, that before the first volume appeared it proved possible to go through the house on “S” Street, where a trunk in the attic contained masses of papers pertaining to Wilson’s early years. The very slight addendums to the present three volumes show that as volumes have been published the editors have missed almost nothing.

Volume 36 begins with Wilson’s speaking tour into the Midwest in support of preparedness, and relates the Pancho Villa raid into New Mexico and the punitive expedition of General Pershing, the second peace mission to Europe by Colonel House, and the crisis with Germany over sinking of the *Sussex*. Volume 37 shows Germany’s giving in to Wilson’s *Sussex* note, continued trouble with Mexico, and preparation for the presidential campaign. Volume 38 traces passage of the Adamson Act, and Wilson’s successful campaign for reelection; it prints all of the campaign speeches and other addresses.

Of individual items in these volumes the speeches are the most interesting, for Wilson, in the words of Editor Link, was “a spellbinder of immense power during an era when Americans admired oratory above all other political skills, and he was irresistible in leadership so long as he voiced the dominant national sentiments.” The president usually outlined his speeches and spoke from the outline—a good college lecturer, he knew the importance of extempore. And what marvelous speeches resulted, almost any of them highly quotable. “A plain gentleman in black—sometimes a very plain gentleman—presides over the military force of the nation, and the thing is symbolic. We think first of peace . . .” (vol. 26, p. 29). “And, then, there seems to rise over the graves of these men and to hallow their memories that blue space of the sky in which stars swim, those stars which exemplify for us that glorious galaxy of the states of the Union—bodies of free men banded together to vindicate the rights of mankind” (vol. 36, p. 85). “If a man with red blood in him had his choice, knowing that he must die, he would rather die to vindicate some right unselfish to himself, than die in his bed” (vol. 36, p. 100). The letters are almost as interesting, such as the terse exchange with Charles Evans Hughes when the Republican candidate for the presidency resigned from the Supreme Court; Hughes resigned in a sentence, and Wilson accepted in two, “to take effect at once” (vol. 37, p. 186). Incidentally, the letters contain very few “may I nots,” such as to Pitchfork Ben Tillman, “May I not send you just a line of appreciation . . .” (vol. 37, p. 481). As for other materials in these volumes, not Wilson’s, House diary entries stand out. Surely that diary should be published. A passage for April 11, 1916, is marvelous in retrospect: “Dr. Grayson came to the train

with me. I told him I was disturbed by the many complimentary articles appearing about me in the press, and I was afraid they might get on the nerves of the President. Grayson said he did not believe anything could shake the President's affection and confidence in me. He said Mrs. Wilson told Miss Bones the other day that Grayson and I were the only two friends the President had who were serving him without a selfish motive" (vol. 36, p. 463). And there are other, less important items, including a letter that Editor Link must have chuckled over as he marked it for inclusion, from Mary Wilson Thompson, wife of an old friend of the president, who wrote on July 30, 1916, against the suffragist agitation: "A woman can be one of the most useful & ornamental creatures in her own sphere, but in Politics she is dangerous, treacherous & revengeful . . ." (vol. 37, p. 503).

Any criticisms of this superb edition are hardly worth mentioning. After reading the editor's intolerance of error I looked for typos, found none, and became sure of it, until one suddenly came into view, a letter from Wilson to Lansing that begins, "My dear Mr. Recratory" (vol. 37, p. 108). It might, however, have been a presidential pun. And perhaps it is too late to abandon the editorial habit of giving all middle names, such as Newton Diehl Baker and Carrie Clinton Lane Chapman Catt. This allows undue distinction to such simply named Brahmins as William Phillips. It raises a question about Mrs. Mina C. Van Winkle, who emerges editorially as Wilhelmina Caroline Ginger Van Winkle.

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LESTER H. BRUNE. *The Origins of American National Security Policy: Sea Power, Air Power, and Foreign Policy, 1900-1941*. Manhattan, Kansas: Military Affairs/Aerospace Historian. 1981. Pp. vi, 218. \$26.00.

Military and diplomatic historians have tended to go separate ways, and accordingly, Lester H. Brune contends, the study of the history of national security policy—policy integrating military and diplomatic considerations—has been torn asunder. Neither military nor diplomatic historians have shown adequate understanding of each other's discipline when the two fields intersect. Brune's purpose is to help make a beginning of American "national security history" that will appropriately combine military and foreign policy.

The purpose is laudable and the resulting book should be read by both of the relevant groups of specialists; but the basic trouble, as Brune's book

itself confirms, lies not with the historians but with the soldiers and diplomats. If military and diplomatic historians do not speak to each other enough, neither did American generals and statesmen before the Second World War. Military men were suspicious of diplomats whom they suspected of a willingness to negotiate away American security, a suspicion that many naval officers, for example, thought the Washington Naval Treaty confirmed. The diplomats on their part created a professional foreign service in no small measure for the precise purpose of separating themselves from military influences. The career foreign service rejected the balance-of-power ideas of Lewis Einstein, a disciple of Admiral A. T. Mahan who served in diplomatic posts for twenty-seven years but never found much favor in the State Department. Foreign service professionals preferred the contrasting ideas of Archibald Cary Coolidge, who thought the United States could best achieve its proper influence in the world through economic measures and diplomatic expertise divorced from the threat or use of military force.

The consequent lack of origins of a coherent national security policy leaves this book groping somewhat awkwardly for a subject. Much of the work diverges from the main title to concentrate on the evolution of air power, and especially on the rivalry between the army aviators, led by Billy Mitchell, who believed air power could achieve decisive military results independent of the traditional services, and the naval aviators who wanted to make aviation an extension of sea power, but who were divided among themselves about how to do it and particularly about the utility of carrier-based planes versus seaplanes. Brune has unearthed much new detail about these debates, but much of it veers away from national security policy into military tactics.

Nevertheless, Brune at least contrives to pull his narrative repeatedly back toward the vicinity of policy issues. An American national security policy integrating military and diplomatic elements in the first four decades of this century and permitting positive action on behalf of American interests overseas would have had to be a policy relying on naval power. The foreign policy implications of Billy Mitchell's air power theories, in contrast, were isolationist. Mitchell's rejection of naval power in favor of bomber aircraft to seal off the American coasts against foreign navies offered a military program for policy makers who thought the Western Hemisphere could be separated as a self-sufficient and defensive unit from the rest of the world. These policy implications of the controversies over air power have not been recognized by most historians as clearly as they might have been, and Brune is at his best in pursuing them.

With this book, Lester H. Brune makes a promising debut as a national security historian. Let us hope that he will move into a time period affording more scope for his ability to link the histories of military and foreign policy.

RUSSELL F. WEIGLEY
Temple University

DAVID A. ARMSTRONG. *Bullets and Bureaucrats: The Machine Gun and the United States Army, 1861–1916*. (Contributions in Military History, number 29.) Westport, Conn.: Greenwood Press. 1982. Pp: xv, 239. \$27.50.

In *Bullets and Bureaucrats* David A. Armstrong highlights the “critical role of bureaucracy” (p. xiv) in his description of the half-century of technological, doctrinal, and organizational development preceding congressional approval of the integration of machine-gun units into infantry and cavalry regiments. Beginning with the Civil War testing of several prototype weapons and the procurement, in 1866, of the army’s first one hundred Gatling guns, Armstrong moves his story forward on two levels. He briefly recounts the machine gun’s evolution from a heavy, hand-cranked device mounted on an artillery carriage to an automatic, portable, recognizably modern weapon. At the same time, in chronologically organized chapters that relate the army bureaucracy’s response to the machine gun, Armstrong explains why American entry into World War I found the U.S. Army equipped with allegedly inadequate, air-cooled Benet-Mercie machine guns, an untried method for integrating them into infantry and cavalry units, and no satisfactory doctrine for their employment in the kind of combat then raging in Europe. To account for such failures, Armstrong indicts “organizational and intellectual inadequacies” (p. 209) of the army’s ordnance department and new general staff.

The product of thorough research (and accompanied by an excellent bibliographical essay and well-chosen illustrations), *Bullets and Bureaucrats* offers a skillful blend of technological and institutional history in a clear yet detailed narrative. When he assigns responsibility for the army’s shortcomings, however, Armstrong reveals the too-powerful influence of an observation offered in the book’s introduction: By “the fall of 1914 . . . machine weapons in unprecedented numbers [had] drastically altered the nature of battle” (p. xiii). Therefore he faults the ordnance corps for not hastening the development of machine guns, even though fabrication of modern models had to await chemical and metallurgical innovations not available until the 1880s, and he censures nineteenth-century infantry and cavalry commanders for not embracing the new weapons,

though they saw little need for the heavy and relatively immobile Gatling gun with its horse-drawn carriage, limber, and caisson because existing small arms enabled their organized regular units to gain the fire-power advantage over their less-disciplined Indian opponents.

With his attention focused on the world war, Armstrong seems to forget also that the army’s early twentieth-century employment tested its armament only in mobile warfare against the irregular forces it attempted to pacify in the Far East and Latin America and that until 1916 few responsible government officials contemplated sending American land forces to Europe. The army, not surprisingly, failed to develop machine guns, doctrine, and organizations appropriate for that conflict—as had most of the European nations—and the small general staff, when it did consider the possibility of war with a great power, quite properly gave priority to resolving more fundamental manpower and policy issues.

Armstrong has nevertheless written an excellent history of the army’s initial experience with the machine gun and thoughtfully speculated on the link between institutions, technology, and doctrine in a way that both reveals the past and provides insight into the current debate over military reform.

JAMES L. ABRAHAMSON
United States Military Academy

JAMES R. SCALES and DANNEY GOBLE. *Oklahoma Politics: A History*. Norman: University of Oklahoma Press. 1982. Pp. xii, 372. \$24.95.

For many years upper-level undergraduate students and graduate students have beseeched me to answer what to them is a burning question: why is their no in-depth history of Oklahoma’s twentieth century? I could point the students to certain textbooks on the history of the state, but the texts generally slight the recent era. I could point to my own period studies or monographs and to the work of a select few other historians, but, in the main, the question went unanswered. Finally, James R. Scales and Danney Goble have tried to fill part of a noticeable void in the historiography of the forty-sixth state.

Oklahoma Politics is a meaty study broken into seventeen chapters. The book opens with a discussion of early politics in the territorial era. The formation of the progressive Oklahoma constitution merits a section as does the grass-roots socialist movement that developed after statehood. Thereafter, in chronological order, the administration of each modern Oklahoma governor is highlighted in separate chapters. The volume concludes by tracing the “Contours of Contemporary Politics,” which carries the political history of the state from the death of Robert Kerr in 1963 to the early 1970s.

Significantly, the authors highlight the trend of more and more voters in a traditionally Democratic state switching to the Republican party. Throughout, the authors attempt to clear up misconceptions about the state's political history. For example, long clouded has been the story of Governor Jack Walton's (1923) fight with the Ku Klux Klan. According to the authors' interpretation, Walton was never sincere in opposing the Klan. He only wanted a smoke screen to confuse his enemies and the general public.

The strengths of this effort far outweigh its weaknesses. Nevertheless, this reader wishes that the volume, published in 1982, had carried the story of state politics through the 1970s to bring it thoroughly up to date. As it stands, the volume virtually ignores the last decade by covering it most generally and, unfortunately, most superficially. Likewise, the authors appear to use little of the research that appeared on twentieth-century Oklahoma in the 1970s. Few works of the last decade are footnoted, nor do they appear in the bibliography.

The above criticism notwithstanding, Scales and Goble have produced a strong effort. The book is excellently researched (the possible exception being the development of the 1970s). It is written with an even interpretation, which is for the most part very objective. Most citations are to primary documents that are buttressed by leading secondary sources where appropriate. Illustrations and useful maps add to both the volume's worth and its appeal. The more than adequate index is a useful, handy reference.

Now when students ask about twentieth-century Oklahoma history, one can say an excellent step has been taken with *Oklahoma Politics*. The book should be examined by all scholars interested in Oklahoma history. One only hopes that some scholars take the effort by Scales and Goble and build on it to produce an in-depth, comprehensive volume on recent Oklahoma that includes social and economic developments.

JAMES SMALLWOOD
Oklahoma State University

EUGENE C. MURDOCK. *Ban Johnson: Czar of Baseball*. (Contributions to the Study of Popular Culture, number 3.) Westport, Conn.: Greenwood Press, 1982. Pp. xii, 294. \$29.95.

When Eugene C. Murdock, now chairman of the department of history at Marietta College, arrived for his job interview in 1955, he inquired about the naming of Ban Johnson Fieldhouse. Now, almost twenty-eight years later, he has included the answer to his question and much more in a well-researched and well-written biography of Byron Bancroft

"Ban" Johnson. Born in Ohio in 1863, Johnson became the founder and dynamic president of professional baseball's American League from 1900 to 1927.

Ban attended several colleges, including Oberlin, Marietta, and the University of Cincinnati Law School, but never graduated from any of them. He joined the staff of the *Cincinnati Commercial-Gazette* in 1886 and became the sports editor. This position, Murdock writes, "provided him with a necessary introduction to the people and the problems of the game" (p. 29).

Johnson was elected president of the minor Western League in 1894 and worked in that capacity until 1899, which served as "his apprenticeship in baseball administration" (p. 30). The following year, with headquarters in Chicago, Ban renamed the league the American League, claimed "major league" status, raided the older, more established National League (1876) of players, and placed strong clubs in traditional National League cities thereby directly challenging their salary scales, attendance monopoly, and media coverage. After much argument and debate, a national agreement was signed in 1903, which created a national commission to govern professional baseball. This commission, composed of the presidents of the National and American Leagues and a third person chosen by the presidents, governed big-league baseball until the arrival of Judge Kenesaw Mountain Landis and the single commissioner system in 1920. As the dominant and most consistent figure on the commission, Ban Johnson became known as the "czar of baseball."

The major part of the book is devoted to Johnson's years of power with the national commission and covers the challenges of player's unions, the sale, ownership, and location of franchises, minor league affiliations, gambling and game fixing, the Federal League competition of 1913-15, and World War I. During these years, by upgrading and dignifying umpires, making the game more respectable for women, and cracking down on rowdy behavior by players on the field, Ban Johnson transformed professional baseball into the "National Pastime." These accounts are followed by chapters dealing with Ban's personal life and his ever-changing friendship with Charles "Commy" Comiskey, the patriarchal owner of the Chicago White Sox. Murdock then uses the final third of the book to explain Johnson's fall from power, the arrival of Judge Landis as the new "czar of baseball," and Johnson's attempt to stay in the game. Although he successfully promoted a boys' minor league and Mexican baseball during his final years as president, Johnson had several feuds with the commissioner, was publicly humiliated on several occasions, drank too much, and suffered from failing health. At this

point, Murdock demonstrates the compassion evident in many biographies when he identifies Johnson's tragedy and biggest mistake as "his failure to leave baseball in 1920" (p. 245). He finally left baseball in 1927 but from then until his death just four years later suffered from diabetes, hardening of the arteries, and poor eyesight.

Murdock has produced one of the better biographies written about a sports figure and in so doing, used all of the available primary material, including extensive interviews with Johnson family members and friends. The events of Johnson's life are skillfully implanted in the history of baseball and American history in general. Like a growing number of other historians, Murdock has demonstrated that sport is more than "fun and games" and that the scholarly study of it and its key personages can provide unique and valuable understanding of American social life and culture. For this reason, the book should be of interest to a wide range of American historians.

JACK W. BERRYMAN
University of Washington

JAMES J. THOMPSON, JR. *Tried as by Fire: Southern Baptists and the Religious Controversies of the 1920s*. Macon, Ga.: Mercer University Press. 1982. Pp. xv, 224. \$13.95.

Southern Baptists were unusually troubled ("tried as by fire") in the 1920s with arguments relating to modern scholarship, the Bible as a literal text, interdenominationalism, social reform, fundamentalism, Darwinism, the presidential candidacy of Alfred E. Smith, and other matters. Cleavages grew more distinct, funding fell short, evangelism was hampered, and advancement was impeded overall. Yet positive results did ensue, or so James J. Thompson, Jr. believes; he is persuaded that the strife brought "a more realistic perception of the world and the Baptist place in it" (p. 210), thereby conditioning the group for "unity, prosperity, and vitality" (p. 215) in years to come.

But Thompson takes pains not to magnify divisions over principles. His own conclusion that "modernism had scarcely touched Southern Baptists" (p. 139) reinforces an earlier writer's estimate that less than 1 percent of the membership "questioned the Christian fundamentals of biblical inspiration and Christ's virgin birth, atonement, resurrection, and second coming" (p. 77). He thus minimizes variances in core beliefs and focuses instead on a faction of unaccommodating, panic-prone challengers, appropriately taking into account rival leadership aspirations, personality quirks, adverse auguries of the age, and resentments churned up by expanded programming on the part of conventions

and associations. He does find that self-advertised "fundamentalist" Baptists deviated significantly from the denominational mainstream in advocating ties with non-Baptist conservatives, in espousing dispensational premillennialism, and in abetting the troublemaking pastor of Fort Worth's First Baptist Church, the Reverend J. Frank Norris. The author disagrees with scholars who have said that the prohibition movement enhanced intradenominational harmony, that it served as a surrogate for broader social reform activity, and that it principally explains Southern church alignments in the 1928 campaign.

Although this study is based on wide research, is well written, and stands as a fine contribution, two problems particularly are worth mentioning. First, the author unduly minimizes denominational discord both before and after the 1920s. In reporting that there "was little fear of modernism" and an "absence of controversy" (pp. 65, 66) among Southern Baptists around the turn of the century, he ignores the bruising debate over Baptist history that forced the retirement of President William Heth Whitsitt from the Southern Baptist Theological Seminary in 1899. Nor should there be references to present-day unity without some acknowledgment of the ongoing contention over biblical inerrancy and the ongoing campaign to curtail Darwinism. Second, the author leaves the impression that the more ardent premillennialists, particularly J. Frank Norris, virtually dissociated themselves from earthly programs to relieve human suffering (p. 55); one reads that Norris undertook "to exorcise the devils of the Depression" (p. 209) through energetic revivalism. Even if the latter is true, it should also be noted that the pastor and his flock provided sleeping accommodations, food, clothing, and other necessities to multitudes of Depression-ravaged Texans.

KENNETH K. BAILEY
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DAVID ALAN CORBIN. *Life, Work, and Rebellion in the Coal Fields: The Southern West Virginia Miners, 1880-1922*. (The Working Class in American History.) Urbana: University of Illinois Press. 1981. Pp. xix, 294. Cloth \$24.95, paper \$12.50.

PAUL F. CLARK. *The Miners' Fight for Democracy: Arnold Miller and the Reform of the United Mine Workers*. (Cornell Studies in Industrial and Labor Relations, number 21.) Ithaca: New York State School of Industrial and Labor Relations, Cornell University. 1981. Pp. vii, 190.

These two volumes focus on struggles in the coal industry, but, aside from that, they have very little in

common. *Life, Work, and Rebellion* examines the development of working-class consciousness in the bituminous coal fields of southern West Virginia during the late nineteenth and early twentieth centuries. *The Miner's Fight for Democracy* focuses on the internal battles for control and reform of the United Mine Workers of America during the 1970s. There is little overlap between the two.

Life, Work, and Rebellion begins in the late 1870s when southern West Virginia was largely agrarian and continues on for more than forty years into the 1920s. By then the local economy was inextricably tied to coal production, which was in turn linked to the vicissitudes of the national economy. What caused the change was demand for high quality coal. It prompted huge amounts of capital investment and brought tens of thousands of workers from the United States and abroad. At the same time, however, came distant control, political corruption, appalling living and working conditions, unionization, and vicious strikes that trampled civil liberties to such an extent that no side could retain any honor. David Alan Corbin tells this provocative story in great detail largely from the viewpoint of miners who, he says, were "probably the most exploited and oppressed coal diggers in the United States." Perhaps. Although the book spans more than fifty years of time, the emphasis is clearly on the twentieth century, particularly the 1910s, which saw the labor-management wars at their worst.

While it is difficult not to be sympathetic to the miners, Corbin has his own war to fight with labor historians. As he notes in the introduction, the failure of contemporaries to understand these miners was "an outrage and an injustice." From there he moves on to critique the institutional labor historians who suggested that labor unions were just another interest group seeking a larger piece of the American pie. In contrast, Corbin, consciously following the lead of Herbert Gutman, E. P. Thompson, and Erik Erikson, develops the hypothesis that unionization, strikes, and violence were an intrinsic reaction of an oppressed working class seeking to burst the bonds that held it. This class had its own inherent culture and identity of which the union was but one manifestation. This approach guides the book, and Corbin declares at the outset that he hopes he has not betrayed the miners' trust.

The Miners' Fight for Democracy is a far cry from *Life, Work, and Rebellion* in more than just time, subject, and place. The author, Paul F. Clark, was a partisan in the events he describes, and he is not, at least outwardly, concerned with class culture and class struggle. He would probably find more in common with the institutional labor historians than he would with Corbin.

The central issue at stake in *The Miners' Fight for Democracy* is the reform of the United Mine Work-

ers. Although Clark's story begins in 1969 with the assassination of Jock Yablonski, the leader of a reform group, the antecedents drifted back to the era when John L. Lewis dominated the union. Although he won great victories for coal miners, his autocratic style, says Clark, eliminated all but the trappings of democratic process. Lewis offered large salaries and perquisites to his cronies, and as he distanced himself from the men he represented they lost control of the UMW. Lewis's successors lacked his skills and charisma, however, and appeared on the verge of losing control when union president Tony Boyle allegedly had Yablonski murdered.

Far from eliminating the reformers, however, Yablonski's death prompted the formation of a group known as the Miners for Democracy, which coalesced around Arnold Miller, a soft-spoken man crippled by arthritis and black lung disease. In 1972, after three years of effort, Miller ousted Boyle in a bitterly contested election. Once in office, Miller reduced salaries and staff and instituted many democratic reforms. But, after a time, he quarreled with his associates, the reform group fell apart, and in an effort to stay in power, Miller seemed to roll back the clock toward the less democratic days. Although he won re-election against his former comrades and old foes, his deteriorating health forced him to resign in 1979. Clark's conclusion is that despite the rollback of some democratic reforms, Miller's accomplishments would have a lasting impact on keeping the UMW a more open, democratic organization.

Whatever the many differences between these two books, they emphasize that a coal miner's lot remains one of struggle. Despite a century of progress that has seen higher wages, shorter hours, and better working conditions won by labor or mandated by government, the industry remains wracked by labor-management strife above ground and assorted perils below. Old ways die hard in this dangerous business. Clark's book makes one wonder about Corbin's class-culture hypothesis. Whatever may be said about class, Clark seems to suggest that both labor and management, divided themselves, are something less than monolithic forces locked in perpetual combat.

JAMES E. FELL, JR.
Harvard University

RAYMOND R. FRAGNOLI. *The Transformation of Reform: Progressivism in Detroit—and after, 1912–1933*. (Modern American History.) New York: Garland. 1982. Pp. vii, 410. \$50.00.

JOHN E. MILLER. *Governor Philip F. La Follette, the Wisconsin Progressives, and the New Deal*. Columbia: University of Missouri Press. 1982. pp. 229. \$21.00.

These two monographic studies share a common interest in the political reform movements that dominated the first half of the twentieth century. Raymond R. Fragnoli's focus is on one local group of Progressives and the organization they created to realize their particular vision of reform, the Detroit Citizens League. John E. Miller's canvas is a much broader one, the legacy of Wisconsin Progressivism as it expressed itself in Governor Philip F. La Follette's political career and his fluctuating, sometimes strained relationship with President Franklin D. Roosevelt and New Deal liberalism. Fragnoli ends his account with the Hoover years of the Great Depression; Miller begins his at that point, concentrates heavily on the Roosevelt years of the Great Depression, and concludes with Phil La Follette's death in 1965.

Fragnoli's account, essentially a case study done in microscopic detail, is a useful reminder of the great variety encompassed by the terms "progressive" and "the progressive movement," as well as the abiding tensions and contradictory goals that such a diversity of reform groups carried within themselves. One encounters here the familiar band of morally outraged Protestant gentlemen determined to restore righteousness to the conduct of civic affairs. The initial attempt resulted in the formation in 1912 of a reform organization appropriately name the Civic Uplift League. Henry M. Leland, the millionaire automobile magnate, was its patron saint, but the driving force of the organization was supplied by its politically astute secretary, Pliny W. Marsh. It was Marsh who first organized a supporting system of committees within the local Protestant churches; and it was he who quickly recognized the inadequacy of this narrow base and the limited appeal of moralistic reform.

The 1913 reorganization, which Marsh conceived and successfully executed, not only changed the organization's name to the Detroit Citizens League, but effectively broadened its base of support. What follows is a detailed account of the league's victories: reforms of the electoral system, the city charter, and the criminal court system, all designed to achieve the evolving goals of honesty, efficiency, economy, and law and order. The sequel is also a familiar one: decline in the 1920s, the loss of pluralistic support, an even greater concentration of businessmen among the league's leaders, and a constricted reform agenda emphasizing the technical administration of government. The story ends with this elite remnant confronting the ravages of the Great Depression with a futile Hooverian response of "we can take care of our own."

In Miller's study, the central issues he concentrates on include Governor Phil La Follette's three terms, 1931–33, and 1935–39; his successful effort in 1934 to establish the Progressive party of Wisconsin;

his failure in 1938 to create a third national party, the National Progressives of America; and throughout these years, La Follette's persistent attempt to understand the Great Depression and devise an effective state and national economic recovery program that would end economic stagnation and restore full employment and prosperity.

Miller makes it clear that Phil La Follette dominated the politics of Wisconsin throughout the 1930s. He was cast in the mold of Progressive leaders: strong-willed, highly individualistic, charismatic, and with lofty political ambitions. Like his father, whom he emulated, his legislative plans were initially opposed by a conservative coalition of Democrats and Republicans. Not until his third term did he achieve an effective majority in both houses, and then he rammed through his entire program in ten days, creating Wisconsin's "Little New Deal" of 1937.

The Depression revived Wisconsin Progressivism from its doldrums of the 1920s. In part, this was due to the special relationship with President Roosevelt that Phil and his older brother, Senator Robert M. La Follette, Jr., enjoyed. Yet, as Miller emphasizes, Phil's attitude toward Roosevelt and the New Deal was "ambivalent" from first to last. This is difficult to understand since Miller argues that Phil was "a pragmatic progressive politician," whose behavior was "essentially consistent with mainstream American politics." In addition, Phil's ideas on economic recovery, starting with his initial emphasis on inadequate mass purchasing power and redistribution of income, and moving to economic expansion while somehow balancing the budget, were not dissimilar to Roosevelt's groping and confused economic course. Perhaps the explanation lies in Phil's presidential aspirations, set in motion by the sharp recession of 1937–38.

Other instructive themes that Miller treats include the demonstrated inability of the states, even one so well positioned as Wisconsin, to achieve recovery—all roads, in this regard, led to Washington; the unsettling transition from Progressivism to modern liberalism; the long political shadow cast by Roosevelt and its effects on those like Phil La Follette who refused to be a subaltern to the New Deal; the tangled complexities of party realignment, a problem shared by La Follette and Roosevelt; and the elusive search for an effective recovery program at a time of transition from neo-orthodox economics to the dimly perceived solutions of Keynes's compensatory public spending. Miller, in short, has produced a first-rate monograph in the best sense, the combination of a close examination of La Follette that also uses him and Wisconsin to illuminate Roosevelt and the New Deal.

ALBERT U. ROMASCO
New York University

THOMAS E. BLANTZ, *A Priest in Public Service: Francis J. Haas and the New Deal*. (Notre Dame Studies in American Catholicism, number 5.) Notre Dame: University of Notre Dame Press. 1982. Pp. xi, 380. \$25.00.

The massive poverty associated with the Great Depression and the emergence of industrial unionism tremendously influenced the shape of American Catholicism in the 1930s. As unemployment skyrocketed, Catholic leaders advocated the use of governmental power to reform the free enterprise system. Taking Pius XI's encyclical *Quadragesimo Anno* as their guide and authority, churchmen like Cardinal George Mundelein, Archbishop Robert E. Lucey, and Monsignor John A. Ryan endorsed economic planning, public works programs, and support for organized labor. In their minds, the ethos of papal teaching meshed well with the politics of the New Deal.

The story of American Catholics and the New Deal, told by such historians as David O'Brien and Neil Betten, is enriched by Thomas E. Blantz's skillful biography of Father Francis Haas. This study traces Haas's career as an educator and church administrator but most fully as a government labor mediator during Roosevelt's presidency.

Haas was one of numerous Catholic priests whose family background, religious training, and personal temperament impelled him toward public service. Exposed as a youngster to the realities of labor politics and schooled in the Ryan interpretation of papal social teaching, Haas found the New Deal ideologically congenial and accepted government appointments to, among others, the National Labor Board, the WPA's Labor Policies Board, and the Fair Employment Practices Committee. More a doer than a thinker, he acted as a "strike doctor" for state and federal agencies, assisting in the settlement of more than fifteen hundred labor disputes throughout the 1930s, and strove to interpret government policy in the interests of organized labor. His contribution lay not in the formulation of New Deal policy but rather in its implementation. Haas's skill in manipulating administrative law to benefit thousands of industrial workers stands as a considerable achievement.

Haas was obviously an adroit arbitrator; the record proves that. Regrettably, Blantz provides only glimpses of him in action. How did this "ace of federal peace-makers" maneuver behind the scenes to obtain settlements? Why did not his prolabor sentiments compromise his role as mediator? Was his priesthood a factor in getting unions and management to bargain in good faith? While Haas's accomplishments are clear, the man as mediator remains elusive.

A fuller estimate of the impact of the institutional

church on the man would broaden the biography. As Blantz suggests, Haas valued experimentation for resolving social problems, particularly in the field of race relations where, Haas argued, "experiment is even more important" (p. 225). Accordingly, as FEPC chairman in 1943 he urged employers to experiment with full racial equality in employment. Yet as bishop of Grand Rapids (1943-53), his administration is judged "unimaginative . . . safe rather than experimental" (p. 239). It is curious that Haas maintained such contrasting approaches toward his governmental and ecclesiastical responsibilities.

Haas stands in a long line of "labor" priests: Yorke, Ryan, Dietz, Boland, Rice, Hillenbrand, McGowan, Higgins, to name only a few. Historians attempting a collective portrait of them are indebted to Blantz for his important contribution.

SAUL E. BRONDER
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College Park

JEROME E. EDWARDS, *Pat McCarran: Political Boss of Nevada*. (Nevada Studies in History and Political Science, number 17.) Reno: University of Nevada Press. 1982. Pp. x, 237. \$8.75.

Patrick A. McCarran served in the United States Senate for nearly twenty-two years, and, although he represented one of the least-populous states, Nevada, he gained a national reputation. Ironically, McCarran, a Democrat, received press attention because he constantly battled Democratic Presidents Franklin Roosevelt and Harry Truman. Some of the issues of confrontation were basic foreign policy, internal security, public power, fair employment, and labor. McCarran carefully used his committee assignments and the consistent backing of Nevada supporters to achieve prominence in the Senate.

Jerome E. Edwards has written a political biography of McCarran that is designed to emphasize only the politician's impact on Nevada state politics. It is one of a series of books on Nevada history and politics, and the limitations of the series's editors are really the main limitations of this book. It seems an impossible task for an author to underplay a senator's two decades in Washington. Consequently, Edwards performs admirably in at least touching on McCarran's role on the Internal Security Committee, his support of Wisconsin Senator Joseph R. McCarthy, and his isolation. The author deserves credit for bringing significant national issues into focus, even though they are viewed through Nevada spectacles.

There is no doubt that McCarran constructed a political machine in Nevada that was designed to

dominate state politics. The tough native-born Nevadan was able to create such an organization after suffering numerous defeats and rejections by his own party. Indeed, his most memorable political battles were with fellow Democrats. Vail and Key Pittman, Edward Carville, Berkley Bunker, and Thomas Mechling were all Democrats who clashed with McCarran as he fought his way to dominance. Pat McCarran used the tactics of his former enemies to stymie challenges from within the party. He could be cold, calculating, and ruthless in his effort to maintain control of Nevada. Willing to cross party lines, McCarran carefully used his patronage to do personal favors for constituents, reward his friends, and punish those who opposed him. Blatantly political, McCarran was constantly embroiled in numerous confrontations with Nevada's other politicians.

The author is successful in portraying the complexities and idiocies of Nevada politics. By using the available primary sources, especially the papers of McCarran, the Pittmans, and Peter Peterson, a prominent Nevada Democrat, Edwards analyzes McCarran's career with incisive objectivity. McCarran actually deserves a complete and thorough biography, and Edwards should use this book as a springboard. Pat McCarran's Nevada legacy is completed. It is now time to analyze his impact on the national scene.

F. ROSS PETERSON
Utah State University

CHARLES B. HOSMER, JR. *Preservation Comes of Age: From Williamsburg to the National Trust, 1926-1949*. In two volumes. Charlottesville: University Press of Virginia, for the National Trust for Historic Preservation. 1981. Pp. xiii, 716; xiii, 717-1291.

Charles B. Hosmer, Jr., who staked out a claim as the historian of historic preservation in the United States when he published *Presence of the Past* (1965), has now firmly established himself as the dominant figure in his chosen field. *Preservation Comes of Age* is a thoroughly researched, clearly presented narrative history of the years when historic preservation in America matured into a highly organized, nationwide movement. Considering its sponsorship by the National Trust for Historic Preservation, the present study is also surprisingly fair minded and ready to examine the failures as well as the successes. Thus, to the probable dismay of many preservationists, Hosmer devotes twenty-seven pages to Henry Ford's Greenfield Village, as opposed to only sixteen pages to New Orleans. Part of the reason lies in the fact that New Orleans's role in preservation really emerged only after 1949, but the space allotment also demonstrates Hosmer's open-minded recognition that Greenfield Village represents an im-

portant alternative approach, even if one that finds little favor among preservationists today. The best example of Hosmer's rendering of the failures is the nearly thirty pages detailing how the National Park Service got trapped into planning and carrying out the Jefferson National Expansion Memorial in St. Louis. A major disaster for architectural history, with the destruction of hundreds of landmark-quality structures, the memorial was "an urban renewal project with a veneer of history used to coat an expenditure for unemployment relief" (p. 626). We now have a thorough and reliable account of how that came to be.

Preservation Comes of Age does not, however, for all its great length, present a comprehensive coverage of its subject. Unlike *Presence of the Past*, which dealt with all aspects of historic preservation in America up to 1926, the present work is quite selective in its coverage. What Hosmer chooses to do, he does very well, but the unwary reader should not assume that he has been told the whole story. *Preservation Comes of Age* is essentially an institutional history, and space given to each topic closely correlates to its institutional significance. Therefore, over a third of the text focuses on the development of preservation theory and programs within the federal government, while the outstanding preservation accomplishments of Stonington, Connecticut, Marshall, Michigan, and a number of other localities are left unmentioned. Hosmer concedes that "the National Park Service's deliberative scholarly approach to restoration policy did not have much effect on state and municipal programs" (p. 1038), but his book is not primarily about influence or even accomplishment out in the field. It is about preservation institutions; institutionally, the federal government bulks very large and, consequently, is thoroughly treated; since the preservation effort in Marshall, for all of its success, has no organizational focus, it falls outside Hosmer's purview.

Does the material really justify such a copious text? Although relatively few people may wish to read the book straight through, only in a few places could it have benefited from editorial paring. In general, Hosmer uses his space to good effect. The cultural significance of Colonial Williamsburg fully warrants the ninety pages Hosmer takes to tell of its conception, growth, and national influence. And while he passed over Marshall, Michigan, Hosmer does explore a large number of relatively out-of-the-way places that deserve a much wider recognition than they have previously received. There is, for example, Mission Le Purisima near Lompoc, superbly restored over many years by the California Park Commission, or Spring Mill Village in central Indiana, where, Hosmer concludes, "even to this day the unparalleled setting distinguishes it from nearly all other outdoor museums" (p. 396). The

sheer length of the book emphasizes the lamentable consequences of historians' general failure to play more than the most peripheral role in historic preservation, a fact that Hosmer repeatedly notes. "Americans were learning to define history in a new way" (p. 377); unfortunately, historians have been extremely slow and grudging in responding to that redefinition.

Preservation Comes of Age will long remain a vital reference work for both historians and preservationists, but using it has been rendered needlessly and inexplicably clumsy by the editors. All notes are at the end of the second volume, requiring a sturdy table to hold both volumes whenever citations are consulted. Although there are 225 excellent illustrations, there is neither a list of illustrations nor any reference to them in the index, making it extremely difficult to find a particular picture. Worst of all, the index is far too brief and incomplete. The entries for Williamsburg, for instance, do not include brief discussions (pp. 351, 538, 926, and elsewhere) of its influence on other projects. We can only hope that the next printing will rectify these errors of omission. *Preservation Comes of Age* is too important a reference work to be so ill served by its publisher.

J. MEREDITH NEIL
Casper, Wyoming

JAMES C. COBB. *The Selling of the South: The Southern Crusade for Industrial Development, 1936-1980*. Baton Rouge: Louisiana State University Press. 1982. Pp. xii, 293. \$16.95.

With this excellent book James C. Cobb adds his name to the growing list of scholars who find that the economic modernization of the South has been as great a force for continuity as for change. Like Jonathan M. Wiener and Dwight B. Billings, who examined the social origins of the post-Civil War "New South" in Alabama and North Carolina, Cobb finds that the promotion of industrial development after World War II followed a similar pattern of conservative modernization. The crusade to industrialize the South in the mid-twentieth century resulted in the emergence of a more urban and vibrant "modern" South, but the process tended to preserve rather than threaten existing power relationships in the region and to sustain Southern traditions of low-wage, nonunion labor, minimal government, and low taxes.

Cobb traces the selling of the South from the Depression era efforts to "balance agriculture with industry" launched by Mississippi Governor Hugh Lawson White to the recent phenomenon of the Sunbelt boom. In 1936 Governor White created the Mississippi Industrial Commission, which sanctioned and supervised the sale of municipal bonds

to finance plant construction. Although these subsidies worked to the disadvantage of small locally owned enterprises, the practice spread throughout the South after the war and was expanded to include other incentives such as tax exemptions, free building sites, and low-interest construction loans. By 1968 all Southern states (except North Carolina) offered industrial bond subsidies as an enticement to industry, and industrial recruitment had become a major responsibility of state and local governments. Increasingly the sellers of the South enlisted the services of key public officials, growth-oriented business leaders, and influential private citizens, as well as a new class of professionals associated with state industrial commissions and chambers of commerce.

The product being sold by the boosters included not only the attractions of climate, markets, and government incentives to industry, but the time-honored promise of docile, nonunion labor. Indeed, Cobb argues, this continued exploitation of cheap Southern labor worked to confirm the economic deficiencies that industrialism was supposed to remedy by guaranteeing the survival of the traditional pattern of low-wage, labor-intensive development. In the 1960s and 1970s Southern states sought to improve public services and to build research and development facilities like the Research Triangle Park in North Carolina in order to make communities more attractive to management and high-technology industry. But these improvements were slow to affect the majority of Southern workers who continued to enjoy fewer benefits from their jobs than did their Northern counterparts. Cobb points out that industrialization did result in the passage of limited regulations to control pollution and industrial hazards, but these laws seldom interfered with state governments' primary role as industrial promoter. During the 1960s, industrial leaders often counseled moderation in racial matters so as not to create a bad image for potential new investors, but blacks seldom benefited equally from economic growth.

The Selling of the South is an important book that breaks new ground in our understanding of the South in this century. It builds on our knowledge of Southern racial and agricultural traditions and asks new questions about continuity and change in the political and economic life of the region. If Cobb is right, the South continues to endure, despite "progress" and its new industrial image.

RONALD D. ELLER
Mars Hill College

PAUL LYONS. *Philadelphia Communists, 1936-1956*. Philadelphia: Temple University Press. 1982. Pp. xii, 244. \$22.50.

This book would appear to have three strikes against it. Paul Lyons is a social worker, not a historian, which may account for his belief that in the late 1940s and early 1950s "all but the most fair-minded liberal scholars succumbed to McCarthyism." Nor does one quite know what to make of his claim that "in the period between the Great Crash and the McCarthy era the CPUSA was the most effective organizing agency within the American experience." This may be either false or meaningless depending on how the statement is read, but in any case one would like to think that a historian could not have written it. Secondly, Lyons is yet another disillusioned New Leftist seeking to understand the movement's failure by examining the history of radicalism, ordinarily a fatal ambition so far as useful scholarship is concerned. A third problem is that while Lyons is well read in the secondary literature his primary research consists of interviews with thirty-six former Communists, too small a sample it might well be supposed.

Even so, scholars interested in the history of the left who pass up *Philadelphia Communists* will miss something worthwhile. Though naive about history and politics, Lyons is alert to the implications of race, gender, and family relations, much more so than the average historian. His chapters on ethnicity and a subsequent one on marriage, family, and sex roles are notably revealing. Lyons deals equally well with the sensitive fact that perhaps as many as half of all party members in the 1930s were Jewish. Of his sample, which was 64 percent Jewish, Lyons says, "most Jewish Communists wear their Jewishness very casually but experience it deeply." In his modest way Lyons takes a fresh look at this and other well-worn topics.

Philadelphia Communists is a responsible book, too. Lyons regards his subjects as decent people but recognizes that the Communist party did immoral things that cannot be justified or explained away. He does not face up to the support of Stalinism by the people he interviewed as boldly as he does their sexism. It remains, we are compelled to believe, a more delicate subject. Nonetheless, Lyons has composed a good little book (there are only 189 pages of text) that manages in places to be both compassionate and forthright. As a result it stands above most of the vast literature on American Communism.

WILLIAM L. O'NEILL
Rutgers University

HOWELL JOHN HARRIS. *The Right to Manage: Industrial Relations Policies of American Business in the 1940s*. Madison: University of Wisconsin Press. 1982. Pp. ix, 296.

World War II, like earlier wars, enhanced the strength and prospects of organized labor in the

United States. The wartime emphasis on industrial production, moreover, gave a special fillip to industrial unionism, which had previously enjoyed only sporadic success. In the auto, steel, machinery, and rubber industries membership boomed, union powers grew, and workers acquired privileges they had seldom dreamed of before. While the end of the war curbed these opportunities, it marked no return to the status quo ante. After 1945 employers became more assertive, demanding greater union responsibility and an even-handed public role in industrial relations. The hallmarks of their resurgence were the Taft-Hartley Act of 1947 and the widespread adoption of a "realistic" approach to industrial relations, one that involved, in essence, the extension of scientific management into the last recesses of old-fashioned, combative, personal, and unsystematic personnel management. By 1950 they had recaptured the initiative if not their former power. The modern era of bureaucratized labor relations, pitting corporate technocrats against labor technocrats, and both against the consuming public, had begun.

Howell John Harris explores the managerial revival of the 1940s with intelligence and sensitivity. He follows the industrial relations "issue" as it was reported in the contemporary press and as readers over the age of forty will remember it. Although he writes little that is unfamiliar to specialists, his essays contain many valuable insights. His analysis of the subversion of supervisory unionism, the origins of Taft-Hartley, the rise of systematic collective bargaining, and the eclipse of "corporate liberalism" are among the high points of the book. If there is any flaw in Harris's treatment of these and other subjects, it is his tendency to whet the reader's interest with one or more provocative statements, then move on to something else. As a result *The Right to Manage* is more often an extended survey of needs and opportunities than a full-scale exploration of industrial relations practices during the 1940s.

Harris is undoubtedly correct when he argues that managers regained the upper hand after World War II, but his analysis suggests that they had slight reason for self-satisfaction. They appear to have been neither astute nor very successful, considering the opportunities of the moment. By the mid-1940s the unions were unpopular with the public, divided ideologically and politically, and with few exceptions devoid of fresh leadership. Yet employers could do no more than implement the stale ideas of the late 1930s and manipulate the National Labor Relations Board. By failing to reverse the pattern of the war period, as they had done after World War I, or perhaps more realistically, to enlist unions in the cause of efficiency and growth, the manufacturers of the 1940s contributed substantially to the forces that would undermine and ultimately destroy much of American industry in the 1960s and 1970s.

Harris rightly does not speculate on these trends, but he might have devoted more attention to changes in the corporate world that indirectly affected his subject. The ascendancy of marketing executives in most big businesses during the postwar years pushed manufacturing and everything associated with it to the corporate back burner. Although industrial-relations staff positions proliferated, they also became dead ends, far from the mainstream, much less the fast track. Despite the executives' public admonitions and frequent cries of anxiety, it is hard to escape the conclusion that they really did not give much thought to their factories or their workers.

Although Harris often uses the terms "business" and "businessman," his study is confined wholly to the manufacturing sector and disproportionately to the automobile industry. It is likely that a different picture would have emerged if he had also examined the transportation industry, where the Teamsters seem to have missed the news of union decline and were expanding toward pre-eminence in the labor movement, or the mining industry, where vast economic and technical changes were transforming the context of industrial relations. *The Right to Manage* is a valuable step toward a full-scale reassessment of the labor history of the postwar era. One hopes it will encourage others to attempt similarly ambitious and well-informed studies.

DANIEL NELSON
University of Akron

RICHARD C. LUKAS. *Bitter Legacy: Polish-American Relations in the Wake of World War II*. Lexington: University Press of Kentucky. 1982. Pp. 191. \$16.00.

This important and well-documented study of interaction between recent diplomatic history and ethnic pressure groups is a welcome sequel to Richard C. Lukas's earlier valuable volume, *The Strange Allies: The United States and Poland, 1941-1945*. What characterizes Lukas's succinct writing is his impeccable research in primary and secondary sources.

Unlike the recent, well-written popular works of Richard M. Watt, *Bitter Glory: Poland and Its Fate, 1918-1939*, and Stewart Stevens, *The Poles*, Lukas follows the professional monographic model on Polish-American relations that characterizes the work by Brożek, Wandycz, Wiczerzak, and this reviewer. They have gradually filled the great gap that hitherto existed in scholarly interpretation of political, economic, and cultural relations between the two countries.

The author covers the incoherent attitudes of Truman's Secretaries of State James Byrnes and George C. Marshall, both insufficiently versed in East-Central European affairs. Despite the emotion-

al commitment on the part of the noble Ambassador Bliss-Lane and his staff (particularly Gerald Keith) and Lane's successor, Ambassador Stanton Griffis, together with a few better-informed American diplomats in Washington (Durbrow, Clayton, Clifford, Elbridge, Gregg, and Hugh Gibson), American policy toward Poland proved to be disastrous because it failed to take advantage of those clauses of the Yalta Agreement that supported Poland's claims and its democracy and thus encouraged the hapless ex-Premier Mikolajczyk to conduct an unrealistic struggle for unfettered elections. At the same time there was a genuine attempt in Washington to follow the example set by Herbert Hoover in post-World War I American Relief Administration assistance.

The author records the foresight of the leaders of the Polish American Congress, Rozmarek of Chicago, Januszewski of Detroit, and the publishers of the *Nowy Świat* daily in New York City, in their demands for political pressure on Moscow and its Warsaw agents, to allow the Poles freely to elect their own government. In view of Secretary Byrnes's Stuttgart speech (September 1946) favoring German revisionism and the inconsistency of Washington's relief and repatriation policies (Poland's regime became officially the largest single recipient of UNRRA), Polish-Americans failed to influence immediately American policies toward Poland and the Soviet Union.

Contrary to the claims of revisionist historians, Lukas puts the blame for the outbreak of the Cold War rather on Communist violations of the wartime agreements in connection with Poland than on the American Polonia and their friends who remained loyal to Roosevelt's original promises of the Atlantic Charter and "four freedoms."

My disagreements with the author are of a minor character and deal with his simplistic branding of the anti-Soviet press and politicians as "rightist" or even "ultranationalistic." After all, among those who objected from the very beginning to any sell-out to Russia's imperialism were the leaders of the Polish Socialist party (PPS), Arciszewski, the Ciolkoszes, Pragier, Puzak, Zaremba, and Zuławski. The latter, by the way, never led "the right wing of the PPS," unless the author believes in Soviet semantics that whoever opposes Kremlin policy becomes *ipso facto* a rightwinger or conservative. To some extent it applies also to his incorrect assessment of the Piłsudskiites, who certainly should not be branded as "ultranationalists." There are a couple of mistakes, such as describing the head of the security ministry, Radkiewicz, as another "Polish Jew" in the Communist establishment, or claiming that the opportunistic ambassador in the United States, Winiewicz, was the wartime employee of the exiled ministry of education (instead of congressional affairs), but worst of all is the confusion in the

footnotes of the two Korbońskis, Stefan and Andrzej, and taking the Stalinist writer W. T. Kowalski for a representative of "Polish historians." These indicate insufficient familiarity with the Polish political spectrum.

Otherwise this is an important contribution toward a better understanding of postwar events in that *terra incognita* of American historiography. And as such, this concise volume well justifies its intriguing title.

GEORGE J. LERSKI
University of San Francisco

EDWIN R. BAYLEY. *Joe McCarthy and the Press*. Madison: University of Wisconsin Press. 1981. Pp. x, 270. \$16.50.

Chroniclers of Joe McCarthy's career commonly pay heed to his manipulative relationship with the press. Edwin R. Bayley, who as a Milwaukee *Journal* reporter once shared the McCarthy beat, has made this his book's central theme. He focuses a critical eye on six aspects of the media's performance in the McCarthy era: the newspapers' handling of McCarthy's initial anti-Communist charges, their coverage of the Tydings subcommittee investigation, the newsplay given McCarthy's role in the 1952 campaign, the particular travails of the wire services, McCarthy's attacks on the "left-wing press," and—far less thoroughly—the role of television in McCarthy's downfall.

Bayley wrestles strenuously with the question of whether journalism's sacred canon of "objective" or unevaluative reporting inadvertently handed McCarthy a potent weapon. His answer is a qualified "yes." Objectivity required journalists dutifully to print whatever Joe said, no matter how patently false. Sloppy headlines that distorted and exaggerated McCarthy's charges compounded the problem. Bayley sympathetically notes how hard it was to do otherwise: journalists—especially for the wire services—faced the pressures of deadlines and competition. Critical analysis in a news story was at the time generally unthinkable. Many among the fourth estate agonized over the way McCarthy exploited "straight" reporting. One reporter for United Press lost eighteen pounds under the strain. Yet some papers, sensing the problem, did expand the interpretive content of their stories. One leitmotif of this study is the notion that the steeling experience of the McCarthy years helped to mature American journalism, making possible the reporting that unraveled the Watergate scandals. Bayley resists the temptation of excessive self-congratulation.

The author deserves a medal for his dogged newspaper research. He perused 129 papers for coverage of McCarthy's first months in the spotlight.

Many readers, he concludes from this immersion, "were often uninformed and sometimes misinformed" (p. 63). The chance for early containment of damage had been muffled. Bayley inspected coverage of the 1952 campaign with equal thoroughness. He also interviewed many reporters; their reminiscences recapture much of the flavor of McCarthy's relations with the press. Joe always enjoyed the company of newsmen; they in turn came to welcome the stories he granted on demand.

While the press obviously played a key role in McCarthy's career, Bayley has not precisely delineated it. His research is anchored in no theoretical discussion of the impact of the press on public opinion. It thus remains plausible, but not proven, that the press shaped public attitudes toward McCarthy. Bayley ranges farthest down this road in arguing that the nature of local news treatment influenced a given area's vote for McCarthy in 1952; he also suggests that the press, by overstating the dimensions of McCarthy's primary win and then slighting the campaign of his Democratic foe, helped make Joe's general election victory a self-fulfilling prophecy.

There are other problems. Long lists of headlines and summaries of newsplay become numbing. Bayley's interviews with reporters yield a bountiful harvest, but his habit of letting all of a journalist's recollections tumble out at once, however tangential to the issue at hand, clutters the narrative. Wider research in manuscript sources would also have been useful.

Nevertheless, Bayley adds much to our understanding of the mutual dependency of McCarthy and the press, offers revealing insights into that relationship, and conveys the frustrations of those who had the task of reporting the McCarthy story. A trove of journalistic lore on a major "story," the book is a welcome addition to the literature.

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STANLEY I. KUTLER. *The American Inquisition: Justice and Injustice in the Cold War*. New York: Hill and Wang. 1982. Pp. xiv, 285. \$16.50.

Stanley I. Kutler's *American Inquisition* seems to be saying, "Yes, Virginia, there is a rule of law, but do not count on it coming down your chimney." This fascinating, readable, well-researched, and thoroughly documented study of Cold War political justice is part of the new wave of historical interest in the high crimes and misdemeanors perpetrated by the government against its actual or apparent political enemies. Others have already detailed the use and abuse of official power in revisionist accounts of

the grand jury, the FBI, the CIA, the Hiss and Rosenberg cases, the Smith Act, and the Communist party prosecutions among others, while Michael Belknap has just recently edited an important survey, *American Political Trials*.

Using the "prism of in-depth case studies" from the "Tokyo Rose" treason indictment to the sedition trail of John William Powell, Kutler skillfully analyzes eight important vendettas. Each of them helps characterize the many dubious purposes and malevolent tactics inherent in using a constitutional process for covertly political ends. With the many documents extracted under the Freedom of Information Act and with his own sure sense of historical perspective and contemporary setting, Kutler has produced an authentic, exciting, consistently interesting narrative that may well be the finest historical work yet published on the political perversion of justice in the United States. And each dimension of the subject, whether it be the intricate, inside politics among Supreme Court members, the self-serving bureaucratic gamesmanship at all agency levels, or the often random victimization of those whose punishment would legitimize questionable policies is equally well told and interpreted.

Time after time the author leaves his audience aghast at the capricious, distressing, awesome, disingenuous, and vindictive violations of fair play and due process that mocked the "very purpose and being" of the legal system. Yet he also denies in his epilogue that any "total inversion" of the system occurred. There was no "darkness at noon." Certainly Kutler has some examples to support his point of view. Ezra Pound's fame and feigned insanity precluded a trial for treason; passport office autocracy eventually succumbed to democratic control, though still capable of guerrilla ambush; some few courageous and independent judges resisted the political prosecutions they confronted; disgrace by disbarment did not taint every radical lawyer; Harry Bridges and John William Powell survived. The law, Kutler claims, has its own autonomy and ideology, and "must in some measure be independent from manipulation and seem just" (p. 245).

But what is a critic who will not carp? When Uncle Sam says, "No more Mr. Nice Guy," what kind of rule of law applies? Are we not left with nothing but the lapses? It might be more useful to see two constitutional systems, one the remnant of the republic established in 1789, the other the partner of the national security state and its imperial executive agencies. In the latter, where secrecy and covert operations dominate and reasons of state are as imperative as in the days of Louis XIV, government can control the truth and access to it. In that system, political justice has existed and still does reign supreme, an essential attribute of sovereign immunity. *The American Inquisition* helps us penetrate old

constitutional myths and see new realities. It gives us the feeling that American dissenters will continue being "repudiated and neutralized" if they challenge official views, foreign or domestic. The country has become a very crowded theater in which government officials seem only to hear the cry, "Fire!"

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ATHAN G. THEOHARIS, editor. *Beyond the Hiss Case: The FBI, Congress, and the Cold War*. Philadelphia: Temple University Press. 1982. Pp. xi, 423. \$24.95.

A collection of essays by writers who appear to view the Cold War period of American history from what one might describe as a left-liberal perspective, *Beyond the Hiss Case*, according to its editor (p. 5), does not focus on the question of the guilt or innocence of Alger Hiss, the onetime official of the United States Department of State who in the latter 1940s was charged with perjury when he denied under oath that he had passed secret documents to a Soviet spy ring. Rather, the editor explains, it moves beyond the Hiss case to an examination of issues involved in what he describes as the abuse of power by the Federal Bureau of Investigation (FBI), the Un-American Activities Committee of the United States House of Representatives (HUAC), and assorted political conservatives to effect the conviction of Hiss and to harass and discredit dissident activists and organizations. The dissident activists and organizations to whom the editor refers include the National Lawyers Guild, seven-term congressman Vito Marcantonio, the American Labor party, and the Communist party of the United States of America.

Beyond the Hiss Case, in truth, is an indictment—in the main of J. Edgar Hoover and the FBI, the HUAC (and of course Richard Nixon), so-called Cold War or anticommunist liberals, the historian Allen Weinstein, and the administration of Harvard University. The indictment is drawn in essays that consider the transparently illegal activities of the FBI (for example, unauthorized break-ins, mail openings, and wire tapings), the longtime harassment of the National Lawyers Guild and Congressman Marcantonio, the palpably improper alliance between the FBI and conservative congressmen and news commentators, the alleged violations of scholarly canons by Weinstein in his widely acclaimed book, *Perjury: The Hiss-Chambers Case*, the rhetoric of anticommunist liberals, and the cooperation by the Harvard administration with the FBI in the latter's surveillance of faculty members suspected of political radicalism.

Students of the past have ample reason to be suspicious of a historical indictment, particularly

when drawn by writers whose apparent ideological convictions permit them to invoke such terms as McCarthyite, smear, neoconservative ideologue, establishment liberal, and reactionary when considering the objects of their indictment. The present book, moreover, strays from the editor's avowed intent of staying clear of the question of Alger Hiss's guilt or innocence, inasmuch as Theoharis himself devotes a substantial part of one essay to that question, and one point of Victor Navasky's sharply written piece on Weinstein's scholarship is, manifestly, that Hiss may have been innocent.

Still, *Beyond the Hiss Case* is not to be dismissed lightly, notwithstanding the fact that its more salient disclosures and arguments have already appeared in print, several of them in the published writings of the editor. The topics taken up in the essays are of indisputable historical importance, and all, save the one that excoriates anticommunist liberals and intimates that such liberals were veritable fellow travelers of the infamous Joseph R. McCarthy, are soundly researched and cogently argued. Of special interest to scholars of the recent past is Athan Theoharis's essay explaining how Hoover devised a clandestine system for the filing and destruction of records that would disclose the FBI's illegal activities. That system, it would appear, was intended to keep incriminating records from the eyes of snooping congressional committees and perhaps attorneys general. It also has kept them, Theoharis observes, from the eyes of enterprising historians.

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ROBERT H. BREMNER and GARY W. REICHARD, editors.
Reshaping America: Society and Institutions, 1945-1960.
(U.S.A. 20/21: Studies in Recent American History,
number 1.) Columbus: Ohio State University Press.
1982. Pp. xii, 403. \$22.50.

This book, the first volume of "U.S.A. 20/21: Studies in Recent American History," might actually be considered the latest number of the long-established and useful Ohio State University Press "Modern America" series. Its editors and thirteen other scholars have contributed essays examining a wide variety of themes in American life during the years 1945-60. The authors and their topics are: Robert H. Bremner on families, children, and the state; Leila J. Rupp on feminism; William H. Chafe on civil rights; Arthur M. Johnson on business; John Barnard on American labor and the Cold War; Thomas E. Williams on rural America; Roland Marchand on popular culture; James T. Patterson on poverty and welfare; Ronald Lora on education and the Cold War; Mark I. Gelfand on cities, suburbs, and government policy; Eugene J. Watts on law enforce-

ment; Kenneth M. Jones on the government-science complex; Gary W. Reichard on congressional-executive relations; and Bernard Sternsher on politics, policy, and ideology. There are some serious omissions, nonetheless. The editors tell us: "For various reasons plans to include separate studies on constitutional developments, ethnicity, and religion did not materialize" (p. xii).

It is impossible to do much more than venture a few generalizations about this topically and qualitatively diverse collection. At a glance, it strikes one as primarily an effort to bring the new interest in social history to a period characterized by the writing of traditional political history and biography, yet many of the social history essays are equally institutional and bureaucratic studies dealing with the development and administration of government policy toward issues that before the New Deal drew scant attention from Washington. As a group, these essays confirm the enlarged role of the federal government in the lives of Americans after World War II.

The contributions also reflect the diversity of viewpoints in the writing of contemporary American political history. At one extreme, William Chafe appears to reject conventional politics and to assume that its practitioners should regard their craft as a moral enterprise. Chafe's perspective leads him quite naturally into what many other historians would consider the mind-boggling assertion that Harry S. Truman's stand on civil rights "was essentially one of routine political expediency" (p. 97). Conversely, Gary Reichard's essay on presidential-congressional relations is so free of ideological baggage that it doubtless will offend some readers as bloodless, however much they may respect its incisive analysis.

One concludes this book with some perplexity about its purpose. The contributions are inconsistent not simply in quality but also in their objectives. Some are rather routine factual summaries of relatively well-known or easy-to-find material. Others contribute little that is factually surprising but attempt a fresh interpretive overview. Some serve as vehicles for an individual's personal research, whether it is central to an understanding of the broader topic or not. One essay is historiographical and speculative. Most of the pieces are useful in one fashion or another—I found those by Williams, Gelfand, Jones, and Sternsher especially noteworthy—but few of them yield new factual knowledge or stimulating perspectives to professional historians. The audience for this volume is unclear.

It is uncertain in fact that *Reshaping America* confirms the assumption that the years from the end of World War II to the election of John F. Kennedy constitute a definable period in American history. A number of the essays devote a fair amount of space to developments before or after

the period they supposedly address. Most leave the impression of viewing a slice of time in a larger continuum.

In many respects the late forties and early fifties were interim years, occupying a gap between two graphic and exciting periods in American history. Yet in other ways—including the establishment of an affluent middle-class society and the consolidation of a pluralist liberal political structure—they did, it seems to me, reshape American life and politics more fully than this book indicates.

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LARRY W. BURT. *Tribalism in Crisis: Federal Indian Policy, 1953–1961*. Albuquerque: University of New Mexico Press, 1982. Pp. x, 180. \$17.50.

The assimilation of Indians into the larger American society has been an underlying though often unstated objective in federal Indian policy since the 1800s. The Indian termination and relocation policies of the 1950s are the most recent of several periods in Indian affairs when assimilation was the acknowledged objective of the Congress and Bureau of Indian Affairs (BIA). Backers of the two policies envisioned a dramatic cutback in BIA services and the eventual termination of the bureau itself. Congress would conduct a tribe-by-tribe review of Indian readiness for full assimilation and, over a period of years, phase out the limited sovereignty federally recognized tribes exercised over their affairs and property. Indians on reservations would be relocated to major cities and provided job training and other services to help them rapidly assimilate in urban America. Indians who chose not to relocate would no longer receive any special federal services, but would not need them because they would have access to jobs generated by private, mostly non-Indian, development of their land and natural resources.

Larry W. Burt uses a variety of documentary, oral history, and interview sources to trace the origin, limited implementation, and ultimate abandonment of Indian termination and relocation. His chapters on the rise of the "terminationists" in the Congress and BIA are particularly informative. Most accounts of the terminationist era in Indian affairs simply suggest that the BIA pursued Indian termination because of unrelenting pressure from a bloc of conservative Western senators and representatives who were interested in the exploitation of Indian land and resources. According to Burt, partisan politics, Cold War nationalism, racism, fiscal conservatism, and intolerance of Indian social and cultural diversity all played significant roles in the evolution of Indian termination policy. He also shows that the bureau exhibited considerable initia-

tive and independent resolve in its pursuit of rapid Indian assimilation.

Burt's book is a good study, but not a complete one. Little consideration is given to the diversity of Indian opinions about termination policy. Indians who favored termination are generally dismissed as "assimilated Indians." In fact, many protermination Indians sincerely hoped termination would bring private investment and economic development to their former reservations. Termination was viewed as an imperfect, but preferable, alternative to continued dependence on unpredictable federal funds and BIA supervision over tribal affairs. Burt's concluding chapter discusses the relevance of the termination era for Indian policy in the Reagan administration, all in less than seven pages. The effort is too telescoped. Nevertheless, the book is well written and should be read by anyone interested in the continuing uneasy relationship between the United States government and American Indians.

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ARTHUR SELWYN MILLER. *Toward Increased Judicial Activism: The Political Role of the Supreme Court*. (Contributions in American Studies, number 59.) Westport, Conn.: Greenwood Press, 1982. Pp. xii, 355. \$29.95.

The historic nineteenth- and twentieth-century debate over judicial review, and the role of the Supreme Court in American government and politics, has gone through innumerable twists and turns in our times. Since the 1940s such attacks and defenses have increasingly constituted political moves designed to affect public policy. The critics of the Warren court, from Birchites to conservative lawyers—Alexander Bickel, Philip Kurland, Raoul Berger, Herbert Wechsler—despite their denials to the contrary, were concerned far more with what the court was doing than with the way the justices were doing it. What they deplored was courts, responding to liberal interest groups—dissatisfied with conventional, middle-class-minded legislatures and their ignoring of minority needs—and assuming law-making and policy-making functions that elevated egalitarianism into a high, if not the highest, social value and standard for constitutional and governmental decision making. Judicial activism in the service of conservative ends, as exercised by justices from John Marshall Harlan III to William Rehnquist, caused them little concern and frequently quiet satisfaction.

The late 1970s and the Reagan years have seen a new flurry of such writing, from neoconservative journalists and pundits to a divergent range of prominent legal scholars: Jesse Choper, John Hart

Ely, Michael Perry, and the West Virginia state legislator turned judge, Richard Neely. Again the issue is the Court's policy-making role, but mercifully, the current renewal seems more thoughtful and explorative than denunciatory and negative.

The participation of Arthur Selwyn Miller in this controversy has been steady and consistent. As early as 1960, he raised justifiable doubts regarding Herbert Wechsler's call for "neutral principles" as the goal for the Supreme Court to pursue; and his steady outpourings have been provocative, especially in their thoughtfulness and use of materials from disciplines other than law to enrich the context of the argument.

In this most recent study, Miller extends themes that he has been developing since the 1950s. For him, the courts have always had a legitimate policy-making role, one not only justified but also highly essential. Courts can breathe fairness and equity into the broader society because they are in a unique position. They have freedom to some degree from the vacuousness of democratic procedures, the log-rolling, and the need to respond to well-organized pressure groups and voting blocs that characterize the legislative process. Thereby they can ameliorate not only some of the worst aspects of majority rule but also the struggle for economic power, which, in Miller's carefully developed argument, is the tail that wags the dog of the allegedly democratic public polity in the United States.

Miller's study is both historical and, to a degree, prescriptive. Looking at the Supreme Court over time, he sees it as, from the start, both a legal and political body. He examines the Constitution, exploring its role and functions, particularly in its economic dimensions. His conclusions are heavily economically determinist. To him, the United States has always been a class society, and the ruling economic class has had an ambivalent relationship with the political Constitution. It has used that Constitution skillfully, and at times shamelessly for its own self-interest, and gearily at the expense of the have-nots in American society. But Miller feels that the institutions of the economic Constitution are not subject to the limitations of those of the political Constitution, particularly due process of law and equal protection of the law. It thus becomes essential that the Supreme Court, as the one body that stands apart from this economic manipulation, be the agency to retain, defend, and extend those governmental processes geared to achieving the fuller realization of human dignity.

Interspersed in the latter portions of the work are pithy observations about pluralism as anarchy; about constitutional fetishism and its role at the core of America's civil religion; and about the need for each generation to write its own constitution. Miller considers the Supreme Court to be the only existing

governmental organ that can articulate national values successfully. He urges increased accessibility to the Supreme Court and the enhancement of a kind of judicial activism geared to helping American make necessary social and political adjustments "as the ecology trap closes and the Age of Scarcity begins."

Generally the study is provocative, but frequently repetitive. The categoricalness of Miller's statements regarding the economic Constitution are at times sufficiently doctrinaire and ahistorical to warrant justified suspicion. But placed against some of the more hyperbolic current critiques of the judicial behavior of the last half century, the work is a useful antidote, filled with intriguing arguments for the activist position.

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STEPHEN B. OATES. *Let the Trumpet Sound: The Life of Martin Luther King, Jr.* New York: Harper and Row. 1982. Pp. xiii, 560.

Let the Trumpet Sound is a big book and a big disappointment. While Stephen B. Oates has had available to him previously closed manuscript and archival materials and received the warm support and cooperation of Coretta Scott King as well as other intimates of Martin Luther King, Jr., the volume provides little new information and adds no illumination to our understanding of his role in the black protest and peace movements. It is so uncritical that it often has the ring of an authorized biography, and it certainly offers no one except novices much in the way of an increase in knowledge about this important figure.

Oates defines himself as a "professional biographer" who aims to remain true to his "storytelling art." He consciously downgrades—even disparages—the analytical mode. Actually, even from the perspective of an old-fashioned biography, *Let the Trumpet Sound* shows a serious weakness arising from the skimpy footnotes that leave many statements undocumented. Moreover, as far as one can determine from his footnoting methods, Oates has depended on David Garrow's summaries of the contents of the FBI transcripts rather than closely examining them himself.

The fact is that *Let the Trumpet Sound* does not offer as much understanding of King as the earlier works by David Lewis and Garrow. Lewis not only provided a strong narrative line but made an astute analysis as well, delineated far more clearly than Oates the meaning and significance of King's transition from race leader to peace leader in his last years, and took a far less uncritical view. Compared to Oates, Garrow's volume on the Selma campaign

did a far better job of portraying the intricate relations between the president, Congress, and King and the Southern Christian Leadership Conference (SCLC)—which Oates treats in a very oversimplified fashion. Moreover, Garrow in his book on King and the FBI supplied a thorough description of the relationship between J. Edgar Hoover's organization and the black leader that Oates fails to match.

In spite of its readable style, *Let the Trumpet Sound* paradoxically is a surprisingly dull book. Tending to be rigidly chronological in organization, it shuttles back and forth in a rather confusing manner. Since Oates aimed to "make his figure speak in [his] own voice and stance," there are frequently long quotations and detailed summaries of speeches. Unfortunately what is lacking are effective shaping and succinct descriptions that would have made the significance of these speeches more readily apparent. Oates asserts that he has written a book that illuminates the times in which his subject lived. Actually he has not done this effectively. For example, not only has Oates been weak in his discussion of King and the political establishment but also provides virtual caricatures of the black leader's relationships with the rest of the civil rights movement.

Essentially, Oates pictures King as more heroic than he probably was and greatly overstates the importance of his role in the movement. While Oates does deal with some weaknesses of King, the overall interpretation is far too favorable to this leader. For example, although Oates does describe the uncertainties that King entertained at times, one simply does not get a real feeling of King's vacillation and indecisiveness. Oates does treat King's relationship with his SCLC staff, yet this treatment is far from adequate. Ralph Abernathy is caricatured to make him seem as if he were on a constant ego trip. The vital part that James Bevel played in maneuvering King into undertaking his Chicago campaign is ignored. The role of SCLC's first executive secretary, Ella Baker, is not adequately drawn or analyzed. The important contribution that she first made to SCLC and her equally vital role later on in encouraging the SNCC leaders to get out from under King's influence are only cursorily handled. Even more glaring is Oates's failure to assess properly the contribution that Bayard Rustin made to King's work and the way in which King mistreated Rustin when the latter became a political liability. Moreover, in regard to SCLC itself, Oates does not shed much light on organizational dynamics or even on the sources of its financing.

Oates is so taken with King as a personality that he distorts the nature of the contribution of King and SCLC. At one point in the book, in fact, Oates states that a public opinion poll showed SCLC to be the most popular organization among blacks—when the

survey had actually demonstrated that King was the most highly regarded black leader but that the NAACP was the most highly rated organization. This misstatement does not seem to be accidental, considering the fact that throughout the book the role of the NAACP and its executive director, Roy Wilkins, is minimized where not denigrated or ignored. Reading this volume, one would think that King's demonstrations alone produced the 1964 Civil Rights Act. Actually it was the astute lobbying orchestrated by Wilkins and Clarence Mitchell, the NAACP Washington Bureau's director, that assured the bill's passage. But if Oates fails to grasp the significance of the NAACP, he remains as bewildered as King himself is pictured concerning the reasons why the black leader could not work well with SNCC. All in all, *Let the Trumpet Sound* scarcely even begins to treat cogently the interorganizational dynamics that are so important for comprehending the contribution that Martin Luther King, Jr., did make.

Oates' volume reveals the limitations in traditional biography from the perspective of modern historiography. The reader who really wants to understand King would still be best advised to depend on the work of Garrow and Lewis.

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MICHAEL B. KATZ *et al.* *The Social Organization of Early Industrial Capitalism*. Cambridge: Harvard University Press. 1982. Pp. xiii, 444. \$37.50.

Michael B. Katz and his coauthors present, in *The Social Organization of Early Industrial Capitalism*, an ambitious attempt to render a statement on class relations and how they were influenced by the modest industrialization that had been experienced in Hamilton, Ontario, between 1851 and 1971. Their focus is broadened to incorporate some comparable evidence of 1855 for Buffalo and rural Erie County. The documentation for their study rests principally on the detailed records that they amassed relating to the social and economic characteristics of persons living in the Canadian city. These data the authors analyze using a variety of multivariate statistical procedures that summarize and classify many of the variables and that measure the strength of a host of hypotheses about the determinants of relationships that their evidence makes it possible for them to document. The results of these analyses are the grist for their theoretical mill, and the significance of this book depends ultimately on how successfully the authors use the latter to refine the former. It is this challenge that distinguishes the

present study from the earlier book by Katz (*The People of Hamilton, Canada West* [1975]) wherein he presented the first fruits of a large and sustained research project.

The central thesis of this volume is that a "two-class model describes not only the objective relations of early industrial cities but the perception of their principal interpreters as well" (p. 25). With this contention the authors explore widely the writings of nineteenth-century commentators and twentieth-century scholars. Capitalist society, they conclude, "possesses two classes because most people share a common relationship to both of its key aspects: the private ownership of capital and the sale of their labour as a commodity" (p. 43). These are the "business class" and the "working class," both of which are said to be influenced by (but neither of which are defined by) the experience of industrialization. Within this framework the authors attempt with some difficulty to assess the influence of the Industrial Revolution in changing the nature of work, and especially in contributing to the demise of artisans who, because their relationship to the ownership of the means of production is unclear from available records, pose a particular problem for the theory. The limited period, twenty years, on which the authors focus serves both to restrict their observation of change and to reduce the apparent need to incorporate it into their theoretical argument. Having chosen to focus on the city at a period of enormous and reputedly significant change, the impression is often given of relationships frozen in time.

The themes around which large portions of the book are organized reflect concerns of persistent interest to Katz. Social stratification, social mobility, transience, and criminality receive significant treatment. Each is examined with reference to its relationships to ethnicity, demographic structure, occupation, wealth, household composition, and changes in these properties from decade to decade. There are, as well, discussions that are less presaged in previous work. One such concerns property, its use value, and its exchange value. Here the central theoretical device of this volume most clearly influences the structure of the presentation. The chapters on youth and early industrialization and on families appealed most to the reviewer's taste, for here the concepts seemed to provide the clearest structures for the discussions, which were less cluttered than elsewhere by the ever-present statistical materials. These chapters present the authors with their greatest success in their interpretative endeavor.

Elsewhere, the reader is more acutely aware of the problems that must be faced in a critical reading. One is the absence of any adequate discussion of methodology. Time and again the reader wishing

advice on these matters must consult the endnotes, only to be referred to other work, often to such fugitive material as working papers. The unfamiliarity of the statistical analyses to many readers adds to the difficulty created by this strategy. Reference is made to discriminant analysis, regression, analysis of variance, an index of representativeness, and multiple classification analysis, among other measures. The reviewer tried without success to match the authors' treatment and use of the multiple classification analysis with discussions of it in several standard statistics texts. One more hurdle ought to be mentioned, and this of perhaps more immediate theoretical significance. The authors frame their work in terms of class, and yet their measure of it is derived from the occupational classifications that also give rise to measures of stratification. The problem is that they present these ideas as theoretical alternatives. Operationally, the issue is not nearly so tidy or clear.

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FRANK H. EPP. *Mennonites in Canada, 1920-1940: A People's Struggle for Survival*. Scottdale, Pa.: Herald. 1982. Pp. xvi, 640.

This is the second volume of Frank Epp's study of Canadian Mennonites. In his earlier work, *Mennonites in Canada, 1786-1920: The History of a Separate People*, he presented a total examination of the Canadian Mennonite experience. Several reviewers recognized the book for its objectivity of interpretation, its insight, and its scholarly approach based on extensive documentation.

The same scholarly method and interesting presentation is applied in Epp's new book, which continues the saga of the Mennonite people in Canada during the dynamic and turbulent period between the two world wars. The twelve lengthy chapters, with subdivisions, deal with their fundamentalist beliefs (Anabaptist) and the resulting conflicts in modern Canadian society, which led some groups to emigrate to Mexico and Paraguay. The uprooting of their colonies in the Soviet Ukraine brought large numbers of Mennonites to Canada, which increased the settlements and congregations and doubled the population in the two decades. Considerable attention is focused on the problems of the Great Depression, the international connection, federation and fragmentation, "keeping the young people," and preserving the culture.

Epp's lucid prose should appeal to a general audience and to scholars. Attractive features of the book are its bibliographic documentation and numerous tables, maps, and illustrations. These make

it of inestimable value not only to the general reader but also to scholars of Canadian history, religious studies, ethnic studies, and, of course, the history of the Mennonite people.

Consequently, this second volume on the *Mennonites in Canada* is a welcome addition to Canadian history and historiography. A German-speaking, but primarily religious group, the Mennonites are indeed unique. This distinctive group scattered preponderantly in rural settlements—although they gradually were urbanized—across Ontario, Manitoba, Saskatchewan, Alberta, and British Columbia, represent a history of pioneering covering almost two hundred years in Canada. The Mennonites are a people characterized by external domination, religious and cultural strife, and internal fragmentation. In this turmoil, which often assumed bitter, disruptive proportions, the Mennonites have had to reassess their values and adapt themselves to intolerable conditions imposed by the federal and provincial governments. Ultimately, it became a matter of their survival, a situation well portrayed in this volume. From the long, turbulent Mennonite experience, Canadians (and this applies equally to Americans) have much to learn by seeing themselves from a different perspective—as ruthless assimilators and even as oppressors. Naturally, this revelation will be shocking to many Anglo-Saxon intelligentsia and leaders.

Attempting to live in isolation in order to practice their faith based on pacifism and nonresistance and to enjoy their traditional way of life, this denomination, an offshoot of the Reformation Anabaptists, an extreme wing of Protestantism, came into conflict with the public school system—the agency of the Protestant majority of Anglicization and assimilation and of Protestant imperialism—particularly during World War II and in the following two decades. It was an unequal struggle for this small Protestant minority, with obvious consequences: the weaker had to leave the country, but the majority remained and gradually adapted themselves. Close communication through local, regional, national, and international conferences provided a transition and subsequently fuller participation in Canadian life.

World War II placed the Mennonites at the crossroads and marked a turning point in their history. Their salvation depended on faith and adaptation for survival. The reader is anxiously waiting for Epp's third volume of the saga of the Mennonites in Canada.

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JUDITH FINGARD. *Jack in Port: Sailortowns of Eastern Canada*. (Social History of Canada, number 36.)

Buffalo: University of Toronto Press. 1982. Pp. 292. Cloth \$35.00, paper \$12.50.

Judith Fingard skillfully and imaginatively uses a wide range of sources to piece together the life and work of nineteenth-century seafarers in the North Atlantic ports of Quebec, Saint John, and Halifax. Her work effectively destroys all the old romantic images of a sailor's lot and delineates instead a class of wage workers with distinctive customs and unusual institutions growing out of their work. As the title clearly indicates, Fingard is not primarily concerned with life before the mast. Rather, she analyzes "the way in which the institutions and demands of society shaped and conditioned the drama of seafaring," and also examines "how shipboard concerns, in turn, fashioned the sailors' experiences on land" (p. 3).

Although Fingard's study spans virtually the entire century, she concentrates on the middle decades when the sailor labor market was internationalized, larger vessels acquired steam-operated winches, working conditions began deteriorating, sailors deserted ships in droves, and boarding-house keepers began their crimping (crew-supplying) activities. In successive chapters detailing the labor market, patterns of seafaring life, rhythms of work and play, the sailor's legal status, and the clash between crimps and middle-class reformers, certain recurrent themes emerge. First, the seasonal nature of work in these northern ports compounded the segregation of sailors, created chronic shortages of labor, and helped spawn both government-run shipping offices and privately conducted crimping. Second, with the exception of the hospitals set up to care for sick and injured sailors, government regulations enacted to "reform" conditions usually worsened them instead. Finally, sailors everywhere earned reputations for rowdiness for good reasons: they were often subjected to excessive discipline, cheated by ships' officers, punished by biased magistrates, and victimized by boarding-house keepers. Fingard shows why Quebec was the worst of these three sailortowns.

Divided by race and ethnic lines, alienated from potential middle-class allies, and isolated on ships for months, sailors rarely reacted in class-conscious ways. Instead they turned to their "friends," the crimps, many of whom lent sailors money and found them jobs while simultaneously exploiting them. By the 1890s, though, steamships were driving sailing vessels off the ocean, longshoremen had taken over loading operations, and the faster ships made briefer stopovers. Both crimps and sailortowns rapidly faded, and few twentieth-century mariners relied on the customs of their nineteenth-century predecessors.

Fingard's method is to present a lucid description

of a general condition, followed by a selection of relevant and often poignant examples. She intersperses these with case studies of individuals like Jim Ward, the Quebec crimp who regularly stevedored on the docks of Savannah, Georgia, during the winter months. To piece together the work routines of sailors, Fingard turns to the logs of vessels stopping at Canadian ports and notes the complaints by ship captains of work *not* being done. Fingard also makes excellent use of court cases and sea chanties to uncover the sailors' own views.

This tightly woven book is a model of diligent research and imaginative exploitation of sources. No historian interested in nineteenth-century maritime history or in social and working-class history can afford to ignore it.

ROBERT H. BABCOCK
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Orono

DESMOND MORTON. *A Peculiar Kind of Politics: Canada's Overseas Ministry in the First World War*. Buffalo: University of Toronto Press. 1982. Pp. xii, 267. \$22.50.

Canadian historians sometimes say that Canada "came of age" on Easter Monday, 1917, when the Canadian Corps and Allied divisions stormed Vimy Ridge on the western front. Canada thereby won military prestige that secured permanent recognition for the Dominion as a nation. Desmond Morton, a prolific author of scholarly books on Canadian military history, substantiates this abstract theory by adding a more specific, but related, aspect. In this book he has detailed how Canada asserted administrative control of its overseas forces during the war. His main theme is the vanity, arrogance, deceptions, and political intrigue of Sir Sam Hughes, minister of militia and defense, a politician who masqueraded as a soldier.

Hughes's administration of Canada's forces overseas was shot through with favoritism for personal friends and political associates, or even for anyone else who applied with the right degree of flattery or obsequiousness, or the right manner or accent. It was marked by vague terms of reference and by conflicting and overlapping jurisdictions. This situation produced an incredible confusion that irritated the British and endangered the interests of those Canadians who were doing the actual fighting while Hughes indulged his neurotic suspicion of the British War Office and military leaders. Ultimately, Hughes's jealous lust for power led to Canada's advantage in the form of autonomy, but not in the way he wanted. Prime Minister Sir Robert Borden created the Ministry of Overseas Forces (MOF) freed from the control of the minister of militia. He

then, belatedly, dismissed Hughes for subsequent blatant insubordination. In a cabinet and parliamentary system of government, the MOF was a peculiar temporary political and administrative expedient, but it was a significant step in Canada's evolution from colony to nation.

Hughes's peculiarities and sins have long been known. Morton gives new and interesting detail about them and makes it even more difficult to understand why Borden put up with him so long. But Morton does not pursue that problem very far, properly so, because it would have taken him into murky smoke-filled rooms in Canada, far from his primary concern of Canadian military administration overseas.

The story of Canada's progress toward military autonomy has usually been told in terms of the conflict over the deployment and integrity of the Canadian Corps between Arthur Currie, the Canadian commander, and Douglas Haig, the British commander-in-chief on the western front. But Currie and Haig remained on professionally cooperative and courteous, even personally friendly, terms. The Canadian military leaders in England did not achieve that with each other. This is a reminder that in war there is often a remarkable degree of magnificent self-sacrificing comradeship in the face of the enemy, but shameful self-seeking and backbiting among those personnel in the administrative tail "who never see the whites of their eyes."

RICHARD A. PRESTON
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BRIAN DOUGLAS TENNYSON. *Canadian Relations with South Africa: A Diplomatic History*. Washington, D.C.: University Press of America. 1982. Pp. xvi, 238. Cloth \$22.00, paper \$11.00.

Surprisingly, this is the first book on relations between Canada and South Africa. Yet the two countries have much in common: both were important self-governing colonies in the nineteenth-century British empire, each with a large non-British population; both were suspicious of schemes for centralized imperial control; and both possessed statesmen, Sir Wilfrid Laurier and Louis Botha, Mackenzie King and J. B. M. Hertzog, who were interested in asserting their country's autonomy. But there was, and is, little substance to the relationship: no large immigration; exports and imports (for Canada) about 1 percent of the total; negligible investment.

Thus it is appropriate that Brian Douglas Tennyson should call his study "a diplomatic history" and that its structure should be essentially episodic. Each chapter deals with a distinct phase in the relationship. The Boer War extended Canada's consciousness, virtually for the first time, beyond the comfort-

able North Atlantic precinct. South Africa became the cause of a bitter debate regarding Canada's responsibility in British wars. A "special relationship" developed after 1902 with Laurier striving, through his rapport with Botha, to reconcile the Afrikaners to their new place within the British empire. Sir Robert Borden and General Jan Smuts in World War I were also allies, although the strength of their partnership has been exaggerated. It was Borden, not Smuts, who wrote most of the famous constitutional declaration of 1917, Resolution IX, and the two differed over Smuts's desire to secure German South West Africa after the war. King and Hertzog were not as close, and, by the mid-1920s, South Africa and Ireland were leading the Dominions toward a more independent status. There was a minor contretemps at the end of the 1930s when the South Africans wanted to send a representative to Canada but did not wish to acknowledge the limitations that the designation "high commissioner" implied. Finally, after recognition had been agreed on in 1939, Canada sent a Belgian economist—not even a Canadian citizen—as its first high commissioner! After the war, the period of "divergent paths" began. Apartheid reared its head, leading, by 1961, to John Diefenbaker's stand against South Africa's continued membership in the Commonwealth. The book ends with Canada's participation in the five-nation "Contact Group" in 1977, which was designed to bargain with South Africa over the future of Namibia. An appendix summarizes the ups and downs of Canada's trade with South Africa since 1901. There is a full bibliography listing primary sources in Canada, Great Britain, and South Africa; government publications; and secondary sources.

This is a valuable and authoritative book, one that is well written, well constructed, and admirably free from polemics. It will undoubtedly remain for many years the standard work on the bilateral relationship.

D. M. L. FARR
Carleton University

LATIN AMERICA

MARVYN HELEN BACIGALUPO. *A Changing Perspective: Attitudes toward Creole Society in New Spain, 1521–1610*. (Colección Tamesis, series A, Monografías, number 76.) London: Tamesis Books. 1981. Pp. 159.

The present slight study in early Spanish American intellectual history is welcome in that, going beyond cataloguing, it examines a certain corpus of material thematically, in order to ascertain the nature and

evolution of certain attitudes therein expressed. The corpus consists of public correspondence (almost all of it directed from Mexico to the crown), insofar as it is available in printed form, as well as histories and belles lettres of the time dealt with, stretching from the conquest into the early seventeenth century. The attitudes are those expressed by royal officials, ecclesiastics, and newly arrived lay Spaniards toward the local Spanish population, as well as the public self-assessment of the locals. Attitudinal shifts over time receive considerable attention.

Marvyn Helen Bacigalupo performs a service by synthesizing this body of opinion within a topical framework and giving us many specific textual examples and references. She breaks new ground with her recognition that much of the public reputation of the native-born Mexican Spaniards of the late sixteenth century derived from earlier lore and polemics relating to the first immigrants or "conquerors." Surveying the corpus en masse, Bacigalupo correctly sees what so many of her predecessors did not, that these pronouncements to the king or the general public are full of convention and posturing for advantage and consequently cannot be taken at face value.

More than attitudes, the author is talking about public stances taken by persons belonging to certain interest groups. To be understood, such positions require a context that the author cannot give us. Her general treatment of sixteenth-century Mexico falls well short of competence. She relies primarily on venerable authorities whose dicta cannot be left unexamined, much less patched together in the fashion of a beginning student. Bacigalupo often betrays ignorance of recent or even fairly recent historical scholarship on early Mexico (although her citations show that she has on occasion peeked at a little of it).

Much of the interest in the book's raw material inheres in the actual terminology used by contemporaries. Here the author's critical bent deserts her; she operates in terms of the modern stereotypes "creole" and "peninsular." "Creole" (*criollo*) was at this time a pejorative nickname, occurring only rarely in Bacigalupo's corpus, and "peninsular" was not used at all. Close attention to the expressions actually in use not only would have thrown light on the important nascent phase of subethnic awareness but also would have shown both unities and dichotomies unrelated to the distinction of Spanish-born versus American-born per se.

Let us have more studies on the general order of this one, but let them analyze contemporary concepts rather than make blanket use of categories that are common coin in today's usage. Let them get beyond the closed world of public statements to the fresher, franker, larger, and far more informative

corpus of litigation, notarial records, and private correspondence. And let them be done by persons who have acquired an adequate comprehension of the past twenty years of scholarship in the field of early Latin American history.

JAMES LOCKHART
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ANTHONY PAGDEN. *The Fall of Natural Man: The American Indian and the Origins of Comparative Ethnology*. (Cambridge Iberian and Latin American Studies.) New York: Cambridge University Press. 1982. Pp. xii, 256. \$39.50.

This book by Anthony Pagden is an engaging contribution to the study of the initial impact of the New World on the Old. It looks at the way sixteenth-century Europeans looked at American Indians, and it explains that comparative ethnology appeared when Spaniards began to compare the varied New World cultures to one another rather than comparing all Amerindians to some preconceived European vision. That switch was adumbrated by Bartolomé de Las Casas, says Pagden, and effectively made by José de Acosta. Acosta classified the Indians, though he thought them barbarians all, into a hierarchy of types ascending toward being civilized. He could do this, we are told, because the School of Salamanca—a group of theologians, most notable among them Francisco de Vitoria—had broken with the dismal view of natural man predominating earlier and had opened the way to a more rational and scientific approach. Vitoria argued that the Indians were not slaves by nature but victims of culture and environment.

Pagden, an admirer of J. G. A. Pocock's insistence on grasping what words mean at a given time, focuses on, as crucial to his theme, the connotations of "barbarian" (and, by extension, "civilized"). He also pays some, too select, attention to Quentin Skinner's emphasis on time, place, and events influencing philosophical concerns. Thus while he is overly hard on the relatively recent scholarship of Venancio Carro and other Spaniards, which is seen as nearly worthless since it seems to Pagden to support Franco's model of a new order, he scants the degree to which the sixteenth-century thinkers he chooses to present tended to support the interest of the Spanish crown. In this conjunction, why dismiss Vasco de Quiroga for his interest in conversion and his disruption of Indian communal organization and not score Acosta for similar goals?

The writing, always gracious, comes especially alive in the discussion of the major role Europeans thought language played in the civilizing process. Less gracious is the dismissal of the first Europeans

in America as simply blinded by their own prejudices. Would it not be more accurate to say that of course they compared Amerindians to what they knew of humankind, but they also repeatedly admitted that their basis of comparison was inadequate to describe the New World to other Europeans? To forget their expressed sense of wonder and acknowledgement of differences, and not to see it as opening the way to explaining them, is unfair to history—to Christopher Columbus; to Hernán Cortés, who could live with ambiguity; to Bernal Díaz del Castillo, who knew a Chichimec from an Aztec; and to many others. In fact, do Vitoria and his fellow schoolmen really deserve more credit for the emergence of comparative ethnology than do Europeans who came to America, not only the sharp-eyed conquistadors but also the friar-chroniclers (among them Pedro de Gante, Andrés de Olmos, Bernardino de Sahagún, and Alonso de Molina, as well as Las Casas and Acosta)? It is here that intellectual history becomes too intellectualized, without a nod to the wider context and the complex interconnections between Old World theories, attitudes, and training, and the European conquerors, settlers, clergy, and officials who knew America at firsthand. This book supplements a body of work by scholars such as Marcel Bataillon, Caro, J. H. Elliott, Lewis Hanke, Benjamin Keen, Edmundo O'Gorman, and J. H. Parry.

PEGGY K. LISS
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MARY TURNER. *Slaves and Missionaries: The Disintegration of Jamaican Slave Society, 1787–1834*. (Blacks in the New World.) Urbana: University of Illinois Press. 1982. Pp. 223. \$25.95.

Protestant missionaries established tentative beachheads in Jamaica at the turn of the nineteenth century. They were received by planters with hostility and trepidation lest Christian teaching and missionary influence undermine the slave labor regime. Denying any intent or interest in altering social institutions, missionaries concentrated on the task of conversion: their objective was to render the slaves Christian, not restive. In order to preserve limited access to the slaves, they were compelled to endure, even to endorse, existing social relations.

It is the central message of Mary Turner's book that Christian teaching subverted the slave system despite disclaimers by the missionaries. Slaves readily perceived contradictions in the Christian message, for example that all men were equal in the eyes of God though on earth gross inequalities persisted. Missionaries failed to comprehend the full impact of their teaching on Native Baptists who drew eclectically from Christian precepts and Afro-Creole reli-

gion to produce a spiritual amalgam that was incompatible with the indignities of slavery. The rebellion of 1831, led by Native Baptists, was catalyzed by the work of missionaries, Turner argues. Their teaching and example afforded slave leaders the will to defy the planters and, if necessary, to endure martyrdom.

Turner's work concentrates on the growth of slave resistance, not on the lives, techniques, and perceptions of the missionaries. Her review of the slave system in early nineteenth-century Jamaica is one of the best accounts in the literature, and chapters treating the "Baptist War" and the defiant heroism of individual slave leaders are poignantly, even elegantly, presented.

But a book in which religion and resistance intersect should probe the spiritual matrix of Jamaican slave society with greater subtlety. Although Turner makes some attempt to identify the African roots of the slaves' beliefs, her analysis of myalist-Native Baptist spirituality lacks the deep-lying qualities inherent, for example, in Monica Schuler's *"Alas, Alas, Kongo": A Social History of Indentured African Immigration to Jamaica, 1841-1865* (1980). This is no mere cavil. Turner is chiefly concerned with the mental chemistry that evoked resistance among slaves. If Christian teaching served as the catalyst for defiance in Jamaica, it is imperative that we know more precisely what was being catalyzed and how the reaction took place.

This point assumes wider importance when the problem is examined in comparative terms. The spiritual harvest of missionaries was substantially greater in the Leeward Islands than in Jamaica before 1834, but it produced no equivalent mood of defiance. Did the widespread Christianization of North American slaves evoke responses akin to those Turner claims for Jamaica? Because Turner ignores the comparative implications of her thesis and probes with insufficient subtlety the underlying spiritual dimensions of slave resistance in Jamaica, her main argument—that Christian teaching led to subversion—remains clouded.

This is, nevertheless, a good book. It is the best treatment of the topic available to date, and it should not be missed by Caribbean scholars of the pre-emancipation period.

WILLIAM A. GREEN
Holy Cross College

MICHAEL M. SWANN, *Tierra Adentro: Settlement and Society in Colonial Durango*. (Dellplain Latin American Studies, number 10.) Boulder, Colo.: Westview Press. 1982. Pp. xxxiv, 444. \$25.00.

Nueva Vizcaya, because it lay between the Mexico City core and the far north, was a "central frontier" of New Spain, "a gateway rather than an end point"

(p. xxi). A region in its own right with natural boundaries to the west and the east, Nueva Vizcaya developed an integrated regional economy motored by its mining sector, which in turn spawned ranching, agricultural, artisanal, and administrative activities. Through the region ran the *camino real*, the main conduit for north-south traffic of New Spain, which created additional needs for supplies, services, and officials.

In *Tierra Adentro* Michael M. Swann has studied the population of Nueva Vizcaya: how it was dispersed, how it was structured, and how it can be described as a society. To accomplish the latter objective he focuses on Durango, the most important city of the region, under the assumption that "regional demographic changes of a long term nature were compressed [there] in both time and space" (p. 395). These three concerns—geography, demography, and social history—comprise the three sections of the book. Their sequence moves the reader from the sixteenth to the nineteenth centuries, from a regional to a neighborhood scale, from aggregates to individual cases, and from mostly descriptive to mostly analytical treatment. With evident mastery of the appropriate disciplinary tools to carry out his objectives, Swann nevertheless remains primarily a geographer in his central organizing idea that "social and demographic processes and conditions. . . . had a spatial expression" (p. xxviii).

The first part of the book treats the establishment and growth of settlements in Nueva Vizcaya. The author schematizes a nine-stage pattern that, with variations, explains the process by which mining communities of northern New Spain evolved. It was not until the eighteenth century, however, that "social, political, and economic links became fully interconnected" in Nueva Vizcaya (p. 37). This interconnection involved a complex range of settlement types—for example, over one-half of the rural settlements of the district of Durango were small ranches in 1777 (p. 77)—that waxed and waned as they were influenced by other variables such as Indian attacks and the revival of mining activity.

The second part of the book treats the demography of Nueva Vizcaya. Here Swann displays greater methodological sophistication than many historians of New Spain (obvious exceptions come to mind such as Sherburne Cook and Woodrow Borah, David J. Robinson and his collaborators, and Claude Morin) who have inferred economic and ecological "causes" for population change without isolating the demographic processes by examining data on marriage, fertility, and mortality. Swann does this at the scale of subregions documenting how varied the impact and timing of epidemic disease and famine were from place to place within the region. By the late eighteenth century Nueva Vizcaya was demo-

graphically complex and had many "frontiers" (mining, mission, agriculture), each with a distinctive population structure.

In the third part Swann examines the society of Durango at two successive moments: in 1778 as a "frontier" city and in 1810 as a matured regional capital fully integrated into the urban network of New Spain. He finds that the "core-periphery" or the so-called preindustrial pattern of settlement had begun to break down by 1810 into a northwestern-southwestern split. In social terms Swann links his discussion of the connections between race, occupation, and social status with an existing literature (after John K. Chance and William B. Taylor, David Brading, and Lyle McAlister). Swann makes an important contribution to that discussion with evidence that neither race nor income correlated with "an occupational definition of prestige" (p. 396).

Swann's research is based on all the appropriate local and general archival collections. Moreover, he has carefully connected his findings to the existing literature in an effort to place Nueva Vizcaya and Durango into a comparative focus of what is known about other regions and cities and against what is known in a general way about New Spain in the eighteenth century. The carefully prepared "figures" (over sixty of them—including maps) are well designed to present a large amount of data clearly and attractively.

In the end a historian might wish that this carefully constructed profile of the population of a region and a city had taken on a bit more flesh by introducing us into the streets and alleys of Durango, thereby telling us of grain storehouses, poor relief, the interests of aldermen, and the functioning of sodalities and guilds; or by showing us the market—its operation and its overlapping hinterlands, problems of sewage and garbage disposal, crime and delinquency; or by letting us see the nightly ritual of the *paseo*, the celebration of fiestas and religious processions, horse races and cock fights. Nevertheless Swann has written a fine regional study that does what it sets out to do very well indeed. Historians of New Spain will profit by a close study of his data and his methods.

RICHARD BOYER
Simon Fraser University

ALEJANDRA LAJOUS. *Los orígenes del Partido Único en México*. (Instituto de Investigaciones Históricas, Serie de Moderna y Contemporánea, number 11.) 2d ed. Mexico City: Universidad Nacional Autónoma de México. 1981. Pp. 268.

The creation of the *Partido Nacional Revolucionario* (PNR) in 1929 was a turning point in Mexican history, for the party (now called the *Partido Revolucionario Institucional*) has ruled Mexico since its

formation. Much has been written on the origins of the PNR but usually in the course of describing the events of the late 1920s and early 1930s. The present work is the first specific and lengthy attempt to explain how and why the party was created and, for that reason alone, merits serious reading.

Alejandra Lajous tackles the problem in two parts. The first seventy-six pages describe the political crisis of 1928 when President-elect Alvaro Obregón was assassinated and incumbent President Plutarco Calles, unable to stay in office without risking civil war, moved quickly and deftly to create a coalition, the PNR, which would allow him to dominate politics while remaining a private citizen. In the second and lengthier part, Lajous shows specifically how Calles used the PNR to create a dual government, one based on the presidency, the other on the party, until President Lázaro Cárdenas used his power, prestige, and cunning to consolidate the two and send Calles into exile. The appendixes are the major party documents discussed in the text and are a valuable addition to the work.

Lajous makes a very strong case that the PNR was the primary mechanism through which Calles maintained himself as the "maximum leader" until 1935. Reading the details of the maneuvering leaves little doubt of that. Scholars have long argued that Calles was the strongman until displaced by Cárdenas. Lajous's contribution is in demonstrating that he did it by creating a dual government. By doing so, she makes a valuable contribution to twentieth-century Mexican historiography.

The book is not well grounded in such traditional historical sources as archival collections and documents written by participants; instead, Lajous relies primarily on three newspapers, a few published memoirs, and scholarly studies. The two archives used are those of the Mexican National Defense Ministry and the National Archives in Washington. The Mexican National Archives are not consulted even though they contain Calles's presidential papers. Lajous draws heavily on the sources and ideas in *Los inicios de la institucionalización: La política del Maximato*, which she wrote with Lorenzo Meyer and Rafael Segovia.

Two negative comments on this otherwise fine book are necessary. It is not a study of the origins of the only party, as the title suggests but of Calles's political ability at a crucial moment in Mexican history. The PNR is not the "only" party, even by the definition of political party she uses.

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JIM TUCK. *The Holy War in Los Altos: A Regional Analysis of Mexico's Cristero Rebellion*. Tucson: University of Arizona Press. 1982. Pp. xiii, 230. \$15.95.

The *Cristero* Rebellion of 1926–29 has been the most critical ideological challenge the ongoing Mexican revolution has experienced since the ideals of the revolution were codified in the constitution of 1917. In that bloody conflict those who defended Catholicism and the traditional role of the church in Mexican society took up arms to resist the implementation by President Plutarco Calles of the severe anticlerical provisions of the constitution.

Polemical works on the uprising have abounded since its inception, but only in recent years has the emotion-filled event been the subject of serious, objective scholarship. This work by Jim Tuck represents a welcome addition to a small but growing body of scholarly literature dealing with the *cristiada*.

Los Altos, the northeastern region of the state of Jalisco, was the seedbed of the *Cristero* revolt. In his introductory chapters Tuck explains the social and cultural characteristics of the region that contributed to the virulent *Cristero* sentiment there. The core of the book deals with the vicissitudes of the rebellion as it unfolded in Jalisco in general and in Los Altos in particular. Tuck has done an excellent job of examining the extant secondary sources and ferreting out previously unknown primary sources. He spent considerable time in Los Altos interviewing surviving *Cristeros* and other eyewitnesses. For information about the rebellion at the national level and the activities of the top leadership Tuck relies on Jean Meyer's comprehensive three-volume *La Cristiada* and, to a lesser extent, David C. Bailey's *Viva Cristo Rey!* The real contribution of this study, therefore, lies in its careful examination of the numerous memoirs of the *Cristero* veterans of Los Altos and the relating of these specific experiences to the overall struggle. Although Tuck is not a professional historian, he has done an outstanding job of judiciously weaving together these accounts and analyzing and evaluating their contradictions and inconsistencies.

Holy War in Los Altos is written in a lively style befitting the professional writer who is its author. He brings alive the flamboyant personalities who lived these events. He sees them as multifaceted people capable of great cruelty, arrogance, and egotism at times, and, at other times, of devotion, humility, and self-sacrifice.

Two minor problems are perceived. One is the practice of quoting speeches as if they were direct quotations when they are actually indirect ones. Also, on page 30 and those that follow Tuck describes the increased restrictions on the church of Calles as taking place in 1924 rather than in 1925. These matters are insignificant, however, compared with the overall value of this fine study.

HUGH G. CAMPBELL
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A. J. R. RUSSELL-WOOD. *The Black Man in Slavery and Freedom in Colonial Brazil*. New York: St. Martin's Press. 1982. Pp. xiii, 295. \$27.50.

This extraordinary book, buttressed by an encyclopedic and disparate superstructure of archival and secondary sources, follows the genre of recent works on slavery by focusing on the almost day-to-day life of the slave and freedman or, as the author characterizes it, nonelite history. In pursuing this mapping, A. J. R. Russell-Wood rips away the penumbra of obscurity around employment options for black slaves and free mulattoes revealing their myriad occupational pursuits in a society that deprecated them as well as their labor and restricted Africans and people of African ancestry to the most menial tasks. But, as Russell-Wood so cogently demonstrates, there were regional exceptions to the above that were determined by the number of whites or the lack thereof in a particular place. In the rural areas, where the European presence was not as strong as in the cities, persons of African ancestry enjoyed greater employment opportunities.

Gold mining provides an example of one of the few routes to prestige and almost instant wealth available to slaves and mulattoes. It afforded the slave with initiative not only the chance to "strike it rich" but also to employ in the competitive world the variety of skills that he had acquired in the "washings."

The lucrative opportunities available to urban slaves, *negros de ganho*, such as muleteering, herding, sailing, serving, carpentering, barbering, masonry, and street selling, among others, led to the creation of "a new social class" (p. 34). This is an intriguing and imaginative statement that provides a challenge to historians of the more contemporary period to chronicle the development, dimensions, and virtual disappearance of this class after slavery. During the 1860s and 1870s the free coloreds dominated the skilled trades and had already entered many professional occupations.

Russell-Wood explains the reasons for the popularity and success of the religious brotherhoods by focusing on their cultural and functional services as well as the sense of community, protection, and identity that they offered.

These brotherhoods were important bulwarks against the vicissitudes of a competitive society whose arsenal of laws and decrees was aimed "at curtailing the social, economic and political freedom of blacks and mulattoes, slaves and free" in order to maintain white superiority (p. 45).

Russell-Wood's latest contribution to Brazilian historiography is an assiduous, careful, and succinct foray into the structure, causes, and dimensions of Brazilian slavery within the parameters of daily life and employment opportunities. He demonstrates

the symbiosis between land and labor and their cultural institutionalizations that commercialized, dehumanized, and consumed slaves, free blacks, and mulattoes.

The suppression or control of African culture and the paranoid fear of and need to control the cultural expressions and movements of blacks are all documented here. This policy, we may note, is still pursued today with respect to political expressions of blackness in Brazil. As long as these cultural expressions are clothed in folkloric and not political dress, they are tolerated.

In sum, few volumes on Brazilian slavery can match this work for its bibliographic contribution and the comparative insights it furnishes into the day-to-day activities of the slave.

ROY ARTHUR GLASGOW
Boston University

PETER BLANCHARD. *The Origins of the Peruvian Labor Movement, 1883–1919*. (Pitt Latin American Series.) Pittsburgh: University of Pittsburgh Press. 1982. Pp. xx, 214. \$23.95.

Unlike the more urbanized and advanced countries in Latin America such as Mexico, Argentina, Chile, and Brazil, predominantly agrarian Peru was not, on the face of it, a particularly likely place to find a dynamic labor movement at the beginning of the twentieth century. But, as Peter Blanchard's judicious, if narrowly conceived, monograph shows, Peru's rapid export-led economic growth after the War of the Pacific (1879–83) formed a crucible on which the country's labor movement was forged. In a scrupulously thorough and detailed manner, which reveals the persistent positivistic overtones of the Anglo-American historiographic tradition, Blanchard documents, largely from a painstakingly careful reading of newspaper sources, the origins of Peru's labor movement.

The locus of early labor activity, logically enough, was the great terminus of the export trade, the administrative and transport center of Lima-Callao, whose founding centuries earlier by Pizarro had radically redefined and reoriented Andean space toward the west. It was there that the first, halting steps toward labor organization were taken in the form of predominantly artisan-organized mutual aid societies, which sought, with modest success, to promote worker goals through participation within, rather than as a challenge to, the established political channels of the oligarchical (Civilist party) state. For Blanchard, the accommodative political posture of mutualism led to its decline after 1908, as periodically deteriorating economic conditions related to Andean boom-bust export cycles created the conditions for a more militant labor response to capital in the form of first anarchism and then syndicalism.

Blanchard carefully quantifies the deteriorating living and working conditions of workers through World War I, culminating in the historic conquest of the eight-hour day in 1919, with an impressive array of statistical materials and tables. Yet here one wishes for a more human depiction of the laboring masses, which only worker autobiographies or the judicious use of period novels can provide. The resort to such innovative sources, the stuff of the new social history "from below" as it were, seems quite beyond the conservative inclinations of Blanchard.

If one detects a predominantly traditional approach to Peru's labor history in Blanchard's work, it is not entirely immune to the broader, innovative currents within the new working-class history. This is demonstrated not only in a fine chapter on rural workers but also in others on the populism of President Guillermo Billinghurst (1912–14) and the industrial action leading to the eight-hour day at the end of the war. Here Blanchard focuses on the distinctions in the labor movement caused by different dominant productive functions (mining, transport, textiles, plantations), various forms of labor (contract or wage, for example *enganche*, *yanacónaje*), the ethnic, racial, and sexual composition of the work force (Indian, Mestizo, black, oriental) and the effect of important secondary cities (Trujillo, Cerro de Pasco) beyond the Lima-Callao export axis on the labor movement. All this is crucial in explaining the problems of labor mobilization in such a heterogeneous, fragmented, and geographically diverse country as Peru.

In the end, however, Blanchard's work is restricted by the boundaries and categories of traditional labor history, with the inherent strengths and weaknesses that this implies. He does not venture very far into the realm of working-class history, which goes well beyond the evolution of trade unions and political action, to a more holistic analysis of working-class culture and consciousness. Perhaps the one type is necessary before the other, and, if that is indeed the case, we are indebted to Blanchard for providing the solid foundation from which we can proceed to a broader discourse on the problem of the formation of the Andean working classes.

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JULIO HEISE GONZÁLEZ. *El período parlamentario, 1861–1925*. Volume 2, *Democracia y gobierno representativo en el período parlamentario*. Santiago: Editorial Universitaria. 1982. Pp. 356.

The author of several well-received works on nineteenth-century Chilean political history, Julio Heise González has contributed a two-volume study of the nation's parliamentary period from 1861 until the

Constitution of 1925. The first tome was issued in 1974 and focuses on the establishment of the parliamentary regime and on the social and intellectual history of the era. The second volume under review offers a detailed analysis of the workings of Chile's parliamentary government and traces the growing democratization of this system.

The work is organized into sections that treat separately the political changes accomplished in each presidential term, the expansion of the electorate, and the role of organized political parties. The author's presentation is thoroughly footnoted and based on a wealth of primary and secondary sources gleaned from decades of research.

Unlike some earlier historians who have argued that parliamentary rule was responsible for governmental inefficiency and ministerial weakness, Heise asserts that this system effectively permitted the dominant forces in Chile to express their demands and to oversee the nation's policies. The consistent curbing of presidential authority in favor of increased congressional powers throughout this period defused political rancor through compromise and institutionalized a respect for democratic forms. In the nineteenth century this meant control by the bourgeoisie intent on establishing a secular state in which the rule of law, personal freedoms, and a policy of *laissez faire* would reign.

Heise maintains that the growing concern for social democracy in early twentieth-century Chile brought an end to the parliamentary system. The widened electoral franchise and increasing labor unrest forced the government to consider the very real problems facing the lower classes. Partisans of state intervention into social and economic affairs believed that an active governmental response was necessary to improve the lot of the poor. These reformers rallied behind Arturo Alessandri, a convincing political orator. He saw the parliamentary system as weak and ineffective and called for a strong executive. Despite entrenched opposition, Alessandri was elected president and in 1925 a new constitution replaced parliamentarianism with a strong presidential system.

Foreign scholars interested in Chile will welcome this careful analysis of the nation's political history. Heise's conclusions on the effectiveness of parliamentary rule in the period 1861–1925 certainly will be received enthusiastically by other Chilean liberals. Supporters of the governing military dictatorship in Santiago, however, will not appreciate the author's evident respect for the growth of democratic institutions and customs during this period of congressional dominance.

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JOHN L. ROBINSON. *Bartolomé Mitre: Historian of the Americas*. Washington: University Press of America. 1989. Pp. x, 117. Cloth \$18.50, paper \$7.75.

John L. Robinson aptly describes Bartolomé Mitre, the subject of his small monograph, as a Renaissance man. Mitre was indeed a man of many talents; he was, with varying degrees of success, a historian, a politician, a poet, a dramatist, a translator, a journalist, and a soldier. Only one of these activities interests Robinson, that of Mitre as historian. Robinson begins his laudatory study by briefly summarizing the life and times of Mitre and then turns to his historical methodology, his historical works, and his contributions to Argentine historiography. The treatment of the Mitre years, 1821–1906, breaks no new ground and contains a number of questionable statements. This is unfortunate, for an in-depth study of Mitre as historian is needed.

Robinson rightly calls attention to Mitre's major historical works, one a biography of Manuel Belgrano, the other a biography of José de San Martín, and to his emphasis on primary sources, documentation, geographical factors, and biographies of great men. That Mitre stressed "objective historical scholarship" (p. vii) is doubtful, for like his contemporaries he had a political aim in writing. His life of Belgrano was written to "awaken the feeling of Argentine nationality" (p. 46), that is, the function of history in his view was to "promote Argentine nationalism," to stimulate patriotic feelings. In this context Robinson never explains what Mitre, the "*porteño* champion" (p. 62), or champion of the city of Buenos Aires, meant by Argentina when he first published his *Belgrano* in 1857, or why Mitre, nearly three decades later, shifted the emphasis in the third edition from Belgrano to the independence movement in Argentina. Mitre's *San Martín* appeared a year later, in 1887, and it stressed the liberation of the South American continent rather than San Martín himself. Why Mitre, the biographer, failed to focus on the Liberator is not clear. Was it because he had never supported San Martín? Another question arises when Robinson states that Mitre in *San Martín* wrote "with a *porteño* or Argentine slant" (p. 97). Does this mean that Buenos Aires is Argentina or that Buenos Aires was Argentina for Mitre? These comments aside, what is seriously lacking is an effort to show how Mitre the politician influenced Mitre the historian, and how Mitre's thinking, both as a politician and a historian, changed over time and was influenced by contemporary events. This calls for a fuller study of Mitre than the one reviewed here. I hope Robinson will undertake the task.

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Collected Essays

These volumes, recently received in the *AHR* office, do not lend themselves readily to unified reviews; the contents are therefore listed. Other similar volumes that are amenable to reviewing will be found in the review section.

JOHN H. HERZ, editor. *From Dictatorship to Democracy: Coping with the Legacies of Authoritarianism and Totalitarianism*. (Contributions in Political Science, number 92; Global Perspectives in History and Politics.) Westport, Conn.: Greenwood. 1982. Pp. xii, 311. \$35.00.

JOHN H. HERZ, Method and Boundaries. JOHN H. HERZ, Denazification and Related Policies. KARLHEINZ NICLAUSS, Political Reconstruction at Bonn. MICHAEL FICHTER, Non-State Organizations and the Problems of Redemocratization. JUTTA-B. LANGE-QUASSOWSKI, Coming to Terms with the Nazi Past: Schools, Media, and the Formation of Opinion. GIUSEPPE DI PALMA, Italy: Is There a Legacy and Is It Fascist? FREDERICK C. ENGELMANN, How Austria Has Coped with Two Dictatorial Legacies. ROY C. MACRIDIS, France: From Vichy to the Fourth Republic. ARTHUR E. TIEDEMANN, Japan Sheds Dictatorship. EDWARD MALEFAKIS, Spain and Its Francoist Heritage. KENNETH MAXWELL, The Emergence of Portuguese Democracy. HARRY J. PSOMIADES, Greece: From the Colonels' Rule to Democracy. JOHN H. HERZ, Conclusion.

DANIEL FREI, editor. *Managing International Crises*. (Advances in Political Science, number 2.) Beverly Hills: Sage. 1982. Pp. 240. \$22.50.

KARL W. DEUTSCH, Crisis Decision-Making: The Information Approach. JEAN A. LAPONCE, Managing International Conflicts: Evidence from Field and Laboratory Experiments. VADIM B. LUKOV and VICTOR M. SERGEEV, Patterns of Crisis Thinking: An Analysis of the Governing Circles in Germany, 1866–1914. JOHN MEISEL, Communications and Crisis: A Preliminary Mapping. RICHARD L. MERRITT, Improbable Events and Expectable Behavior. ALI E. HILLAL DESSOUKI, The Middle East Crisis: Theoretical Propositions and Examples. DANIEL FREI, Escalation: Assessing the Risk of Unintentional Nuclear War. HAYWARD R. ALKER, JR., Collective Security-Seeking Practices since 1945. KINHIDE MUSHAKOJI, The UN System: Structural Transformation

and Crisis. RADOVAN VUKADINOVIĆ, Nonaligned Countries in Conflict. KARI MÖTTÖLÄ, Systemic Crisis: Lessons of Regional Détente. KING-YUH CHANG, Practical Suggestions for Crisis Management: An Inventory. ROBERTO DE O. CAMPOS and NIELS HANSEN, Views from Diplomatic Practice.

RICHARD P. TUCKER and J. F. RICHARDS, editors. *Global Deforestation and the Nineteenth-Century World Economy*. (Duke Press Policy Studies.) Durham, N.C.: Duke University Press. 1983. Pp. xviii, 210. \$35.75.

MICHAEL WILLIAMS, Ohio: Microcosm of Agricultural Clearing in the Midwest. THOMAS R. COX, Trade, Development, and Environmental Change: The Utilization of North America's Pacific Coast Forests to 1914 and Its Consequences. DENNIS M. ROTH, Philippine Forests and Forestry: 1565–1920. WARREN DEAN, Deforestation in Southeastern Brazil. J. F. RICHARDS and MICHELLE B. MCALPIN, Cotton Cultivating and Land Clearing in the Bombay Deccan and Karnatak, 1818–1920. MICHAEL ADAMS, Colonization, Commercial Agriculture, and the Destruction of British Burma in the Late Nineteenth Century. RHOADES MURPHEY, Deforestation in Modern China. MASAKO M. OSAKO, Forest Preservation in Tokugawa Japan. RICHARD P. TUCKER, The British Colonial System and the Forests of the Western Himalayas, 1815–1914. JAMES J. THOMSON, The Precolonial Woodstock in Sahalien West Africa: The Example of Central Niger (Damagaram, Damergu, Air).

PHILIP DODD, editor. *The Art of Travel: Essays on Travel Writing*. London: Frank Cass; distributed by Biblio Distribution Center, Totowa, N.J. 1982. Pp. 164. \$17.50.

JENNY MEZCIEMS, " 'Tis Not to Divert the Reader": Moral and Literary Determinants in Some Early Travel Narratives. JOEL J. GOLD, The Voyages of Jerónimo Lobo, Joachim Le Grand, and Samuel Johnson. PETER MILES, A Semi-Mental Journey: Structure and Illusion in Smollett's *Travels*. F. S. SCHWARZBACH, "Terra Incognita": An Image of the City in English Literature, 1820–55. MARGERY SABIN, The Spectacle of Reality in *Sea and Sardinia*. MARTIN STANNARD, Debunking the Jungle: The Context of Evelyn Waugh's Travel Books, 1930–39. PHILIP DODD, The Views of Travellers: Travel Writing in the 1930s. JOHN THIEME, Authorial Voice in V. S. Naipaul's *The Middle Passage*.

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EVERETT, The Rise of Jewish National Politics in Galicia, 1905–1907. JOHN-PAUL HIMKA, Voluntary Artisan Associations and the Ukrainian National Movement in Galicia (the 1870s). MARTHA BOHACHEVSKY-CHOMIAK, Natalia Kobrynska: A Formulator of Feminism. PAUL R. MAGOCSI, The Language Question as a Factor in the National Movement in Eastern Galicia. LEONID RUDNYTZKY, The Image of Austria in the Works of Ivan Franko. PAUL R. MAGOCSI, Bibliographic Guide to the History of Ukrainians in Galicia, 1848–1918.

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Communications

A communication will be considered only if it relates to an article or review published in this journal; publication is solely at the editors' discretion. Letters may not exceed seven hundred words for reviews and one thousand words for articles. They should be submitted in duplicate, typed double-spaced with wide margins, and headed "To the Editor."

ARTICLES

TO THE EDITOR:

In his "Toward a Social History of the October Revolution" (*AHR*, 88 [1983]: 31–52), Ronald G. Suny raises some provocative questions about the interplay of political and social history as well as the scope of leadership and accident in shaping the course of these events. He may, however, have permitted himself an "ideological intrusion" as arbitrary as those he imputes to the school of political history (p. 32), and in the process he has seriously misconstrued my *Red October*.

Professor Suny alleges that writers including myself who have focused on the actions of political leadership deny the importance of "the great social and economic forces at play in 1917" (p. 41). This is absurd. I let those facets of the Revolution speak for themselves while I concentrated on the story of the Bolsheviks' revolutionary planning and lack thereof, which had been so badly distorted by Communist and anti-Communist mythology alike. Why Suny thinks such an interest must necessarily be "conservative" is beyond me, unless anything that calls into question a radical's faith in the masses is conservative. The encouragement of research on the social history of 1917 (of which Suny has provided a useful compendium) will hardly bring balance to the study of those events if it means that political history is made politically unacceptable.

Thanks perhaps to his bracketing of "manipulation" and "accident" in the negative category of

political explanation (p. 31), Professor Suny has completely misread my analysis of the October events and imputes to me the very stereotype of Lenin—the master manipulator—that I was at pains to refute. While I could not fail to note the force of Lenin's fanaticism and its impact at certain points, the central thesis of my book (so irritating to the Soviet reviewers who have grasped it quite clearly) is that Lenin had failed to get the Bolsheviks to respond to his demand for an armed uprising before the Second Congress of Soviets, until Kerensky's attempt to close the Bolshevik printing plant on October 24 unexpectedly precipitated just such an outcome. As I wrote in *Red October*, "The facts of the record show that in the crucial days before October 24th Lenin was not making his leadership effective. The party, unable to face up directly to his browbeating, was tacitly violating his instructions and waiting for a multi-party and semi-constitutional revolution by the Congress of Soviets. Lenin had failed to seize the moment, failed to avert the trend to a compromise coalition regime of the soviets, failed to nail down the base for his personal dictatorship—until the government struck on the morning of the 24th of October. Kerensky's ill-conceived counter-move was the decisive accident" (p. 216).

Where Professor Suny attempts to quote Alexander Rabinowitch's *The Bolsheviks Come to Power* (1976) against me on this point (p. 43), Rabinowitch is only following, quite accurately, the very argument I had earlier made myself. I noted in my own review of that book (*Canadian-American Slavic Studies* [1978]), "Professor Rabinowitch's work substantially supports, with additional documentation, my own thesis in *Red October* of the hesitations and improvisations that served in place of decision and plan in the Bolshevik accession to power."

Professor Suny maintains that an "accidental" view of these events is ruled out by the revelations of social history. Without minimizing the surge of popular support that had given the Bolsheviks control of key soviets and military units, I demonstrated through a day-by-day review of the docu-

mentary record—that is, political history—the accidental and unplanned *manner* in which this support was translated into Bolshevik power. It was only by chance that the transfer of power to the soviets took place under the circumstances of violence that Lenin wanted, with all the consequences this entailed, rather than simply by “a telephone call” (in B. V. Stankevich’s words, quoted by Suny). There is good reason to believe that the latter approach would have satisfied most of the Bolshevik leaders, that indeed they were waiting for it, even though it implied a coalition with other soviet parties. (On this point, see Michal Reiman, “Trockij 1917, die Geburt einer historischen Persönlichkeit,” in Francesca Gori, ed., *Pensiero e Azione Politica di Lev Trockij* [1982].)

It is important to recognize this possibility of a nonviolent assumption of power by the soviets, in order to appreciate the availability of more than one form of power in October attuned to the social movement and the popular faith in the soviets. One does not need to downplay the turmoil in Russian society in order to argue as I did that the accidental sequence of events on October 24 and 25 ruled out the alternative of an “all-socialist regime” and left only Lenin’s monocratic answer to the revolutionary clamor. Professor Suny desires to put this question of “a dictatorship of the Bolshevik Party” versus “socialist democracy” “beyond the limits of this essay,” but in fact it is crucial to his essay, because it shows how the locus of decisiveness can move out of the realm of social abstractions and into that domain of political events, personalities, and accidents that he strives so hard to denigrate.

It has always been hard for the rational mind to accept the idea that tremendous and irreversible consequences could be set in motion or switched from one track to another by a fortuitous event. What happened on October 24, 1917, was only one link in an intertwined chain of causality and contingency—but it is one point where the documentation shows beyond a reasonable doubt that the path of violence was determined by a throw of the dice and that the path of temporizing—however long or short its consequences may have been felt—failed on the same throw.

Yet, if one does not concede that accidents and individual “political” action can affect the course of the revolutionary process at one point or another, one would have to attribute to the mass social movement the entire totalitarian outcome of the Bolshevik takeover that Professor Suny understandably deplores. Perhaps the question is only at what points in the process ideological preconceptions permit one to recognize the decisiveness of “political” events and wrong choices.

ROBERT V. DANIELS
University of Vermont

TO THE EDITOR:

Ronald G. Suny (“Toward a Social History of the October Revolution,” *AHR*, 88 [1983]: 31–52) was probably right to say that much Western writing on the Bolshevik takeover suffers from political bias. But it would be a pity if radical social historians, reacting against the errors of their conservative or liberal counterparts, were to resurrect some hoary myths about the nature of Bolshevik popular support in the fall of 1917. Lenin’s party, we are told here, offered “the most consistent class interpretation of the revolution,” which appealed to workers developing “a greatly heightened sense of class” (p. 48).

This sort of thing prompts four fairly obvious points. First, the Bolsheviks (or, more properly, the maximalists of every stripe) recruited significant support from many social groups besides workers—notably from soldiers and sailors. Second, this support had more to do with the maximalists’ promise of a speedy peace than with their vague ideas about socialism. Third, disadvantaged people may well express their dislike of the social elite in fashionable Marxist terminology without necessarily becoming class conscious in a Marxist sense. Fourth, such class consciousness as existed among Russian proletarians at this time was fragile and ephemeral: in a matter of months it either evaporated or became subsumed into state consciousness—that is, loyalty to the new “Soviet” order.

The process whereby the revolutionary activists of 1917 turned into the commissars of 1918, ruling through organizations that had lost touch with their popular roots, is described in my *Russian Revolution: A Study in Mass Mobilization* (1977). Suny summarized the argument fairly but did not explain why he rejected it. Bolshevik party dictatorship, he wrote, resulted from civil war pressures (p. 51). Yet the Leninists silenced their socialist opponents within weeks of seizing power, the bureaucratization of the soviets began in the spring of 1917, and the Leninist vanguard theory of revolution was formulated in 1902. Are we to assume that all this was irrelevant? The “mainstream” interpretation cannot be dismissed as casually as it is here. (There is no reference to such standard authorities as Leonard Shapiro, Oskar Anweiler, or even E. H. Carr.) Perhaps a definitive verdict on “Great October” cannot be delivered either by political or social historians but only by moral philosophers with first-hand experience of the system of total power to which it gave rise. Ronald Suny might ponder George T. Peck’s observation, made in a different context, that “materialistic determinism, whether of the Marxist or merely the pedestrian sociological variety, has run out of steam.”

JOHN KEEP
University of Toronto

PROFESSOR SUNY REPLIES:

In reading Robert Daniels's *Red October*, I am still impressed by the exciting narrative that he wove, still stirred by the drama he evoked, but still dismayed by his rather narrow focus on personalities to the relative exclusion of the numerous worker and soldier participants. In his letter Professor Daniels replies to my claim that he "essentially ignored the role of the workers" by contending that he "let those facets of the Revolution speak for themselves." Perhaps, but not in this particular book. He is upset that I have labeled the approach of Melgunov and Daniels "conservative." By using the term "conservative-accidentalists" I meant to draw a connection between views of revolution that underplay the role of the mass of lower-class participants and the political judgment that revolutions are artificial creations of cynical politicians rather than the genuine expression of popular aspirations. When a revolution is conceived as produced primarily by manipulation or lucky accidents, its "legitimacy" as a genuine political response to social conditions is in this way brought into question. But my criticism of an approach that largely eliminates the workers and soldiers from center stage is not only that it can lead to a skewed political evaluation but also that in the case of 1917 it renders almost impossible an explanation of Bolshevik success, unless one contends that it was just a matter of luck.

Professor Daniels is correct to point out that both he and Alexander Rabinowitch have acknowledged the importance of the "decisive accident" (Daniels's phrase) of Kerensky's decision to act first against the Bolsheviks in October. But for Daniels the move by Kerensky, which galvanized the Bolshevik leaders to seize power before the Congress of Soviets convened, as Lenin had been urging, was undertaken as a "wild gamble" without much chance of success. "The stark truth about the Bolshevik Revolution" for Daniels "is that it succeeded against incredible odds in defiance of any rational calculation that could have been made in the fall of 1917" (*Red October*, 215). Although Daniels reported that units in the garrison, even the last bastion of governmental support, the Cossacks, had refused to come out when called by Kerensky, he concluded that the Bolsheviks were not in a strong position to take power and had to be goaded by Lenin to risk everything because there was no other way for Lenin to realize his plans for a one-party dictatorship. Rabinowitch, on the other hand, has seen both the uprising and its success as far from accidental and much more as the result of the deep hostility toward the government and the broad support for Soviet Power among workers and soldiers in the city. In this account, Lenin was genuinely alarmed about government activities and worried that the

party would delay too long and "lose influence among the masses and become powerless to halt Russia's slide into anarchy" (*The Bolsheviks Come to Power*, 179). A long social process had created conditions making an overthrow of the Provisional Government likely. The preparations by the Military-Revolutionary Committee of the Petrograd Soviet were based on "a realistic evaluation of the prevailing correlation of forces and popular mood" (*ibid.*, 267).

Whether one emphasizes accident or manipulation, excessive concentration on the central importance of Lenin has the unfortunate effect of underestimating the coincidence of popular anger and fear with the Bolsheviks' antiwar, antibourgeois political rhetoric and their leadership of the movement for a government of the lower classes. It results in linking the outcome of the October Revolution with the mind of the man who "made" it in a far too simple reduction of a complex social historical process. Neither the October Revolution, as I have tried to suggest in my essay, nor the eventual transformation of Soviet democracy into Stalinist tyranny can be so intimately tied to the ideas of one man or the form of political organization he preferred, as has been done in much of the traditional historiography. My contention is that neither a political history divorced from social context nor a social history devoid of politics provides the integration of social, personal, and political factors required to explain October.

Bolshevik success can be fully explained only by an analysis of the party's ability to establish a "dynamic relationship" (Rabinowitch's phrase) between its cadres and the masses, by considering the impossibility of maintaining a Coalition Government in which conflicting social interests failed to coexist, and, finally, by appreciating the intensifying social polarization that was leading the lower classes toward a radical rejection of collaboration with people of property. Although no one should ignore the influence of accident or manipulation in historical events, I cannot agree that "it was only by chance that the transfer of power to the soviets took place under the circumstances of violence that Lenin wanted." Nor can I agree that by seizing power before the Congress of Soviets "only Lenin's monocratic answer to the revolutionary clamor" was left. For someone like Professor Daniels, who proposes that history is made up of a series of accidents, the events of October 24–25 seem to have an extraordinary and lasting power of determination. I would suggest that the whole history of the Russian Civil War can be seen as the violent working out of political alternatives, left and right, in the process of which Soviet rule metamorphosed into something quite different from what it had been in October 1917.

Professor Keep questions whether the concept of class consciousness is appropriate for 1917. In his view, a sense of class is belied by Bolshevik support from groups besides workers and by that support having more to do with desires for peace than for socialism. The use of Marxist terminology by workers does not, he asserts, necessarily indicate class consciousness in a Marxist sense. Moreover, such consciousness became state consciousness in a short time. My own reading of the evidence of working-class consciousness in 1917 is that more and more groups of workers—along with soldiers, sailors, and others—were developing a sense of closeness to other members of the *demokratiia* ("lower classes") and of distance, social and political, from the *tsentsovoe obshchestvo* ("propertied classes"). This sense stemmed from a variety of sources: from prewar experiences of tsarist rule and treatment at the hands of employers, from the enormous costs of a war that many liberal intellectuals and industrialists continued to support, as well as from the special experience of the year of revolution—the failures of the Coalition Government, continuation of the war, fears of counterrevolution, and socialist propaganda that interpreted social reality in class terms. This "class" consciousness was broader than the working class itself and might better be referred to as "plebian consciousness." It certainly entailed a feeling of "us and them." The Provisional Government was "theirs"; the soviets were "ours." Once the state power was in the hands of the soviets, the transformation of this consciousness into support by part of the *demokratiia* for the new order was not long in coming. As recent work by William G. Rosenberg shows, however, the growing cohesiveness of the lower classes, particularly the workers, evident in 1917, began to break down to some extent in early 1918, a process that may account in part for increased Bolshevik reliance on state coercion.

Both Professor Daniels's and Professor Keep's letters raise broad and important issues not only about the Russian Revolution and its outcome but also about the need to integrate history from above and below. They confirm my view that, try as we may, it remains nearly impossible to separate a discussion of 1917 from the subsequent (though not necessarily consequent) phenomenon of Stalinism. Both writers seek an answer to the question of the source of dictatorship, yet I remain reluctant to overload 1917 with responsibility for a political structure that had barely begun to evolve in that year. I am convinced that the most appropriate place to seek a source is not in the events of 1917, certainly not in the pages of *What Is To Be Done?* (1902), but in the tragic complexities of the post-October period, in the almost complete social disintegration of the Civil War, and in the attempt by the

Bolshevik party to improvise a new political authority.

RONALD GRIGOR SUNY
University of Michigan

REVIEW ESSAYS

TO THE EDITOR:

It is surprising to see a statement by Theodore S. Hamerow ("Review Essay—Guilt, Redemption, and Writing German History," *AHR*, 88 [1983]: 53–72) to the effect that A. J. P. Taylor had in 1945 insisted that the Third Reich was "founded on terror and unworkable without the secret police and the concentration camp" (p. 57) but that in 1961 "he suggested that it was really quite similar to the Western democracies."

Professor Taylor's book, *The Origins of the Second World War* (1962), while emphasizing the supposedly traditional German nature of Hitler's foreign policy in the 1930s, was certainly not an apology for the domestic actions of the Nazi regime nor for its occupation policies. Professor Hamerow's unfortunate choice of words does Taylor's work a great disservice, whatever its merits in regard to the diplomatic history of Europe.

ROBERT E. MOIR
Rumson-Fair Haven
Regional High School
Rumson, New Jersey

REVIEWS OF BOOKS

TO THE EDITOR:

Richard L. Greaves, in his review of Dewey D. Wallace's *Puritans and Predestination* (*AHR*, 88 [1983]: 388), made a somewhat startling reference to "extreme revisionists such as Patrick Collinson." The point at issue, "the recent attempt . . . to paper over the differences between Puritans and Anglicans," refers back to Collinson's *Elizabethan Puritan Movement*, published in 1966 and, therefore, no longer particularly recent. Unlike some others who are reluctant to use the word "Puritan," Collinson, as he recently reminded us in the *Journal of Ecclesiastical History* (1980), is still willing to talk about "Puritans." What he is more reluctant to do is to allow, by the use of the anachronistic word "Anglican," the claim that non-Puritans were more part of the main stream of the Church of England than so-called Puritans.

Since this work was published in 1966, I am not aware that any significant dissent from it has ap-

peared in England, while the work of others, such as Shiels and Lake, has done a good deal to support it. So far as I am aware, the views Professor Greaves dismisses as "extreme revisionist" are barely even controversial in England.

I am writing, not merely to put right a point of scholarly fact, but because Professor Greaves's words seem to me to indicate an unhealthy potential for schism between members of the profession on the two sides of the Atlantic. Greaves's position would force him to dismiss almost all work done in England on the sixteenth and seventeenth centuries as "extreme revisionism." If scholars think most of the work done in England is wrong, it is always open to them to publish a detailed, factual refutation, from which a new dialogue might begin. What is not good for the health of the profession is the tendency to dismiss work done in England as too misconceived to be worth attention. If transatlantic dialogue is to be resumed, it is to be hoped that those taking part will grow a little better able to identify what is genuinely extreme from what is so much in the main stream as to command almost universal assent.

CONRAD RUSSELL
Yale University

PROFESSOR GREAVES REPLIES:

Unfortunately, Conrad Russell has made an erroneous inference from my review and thus tilts at windmills. As he recognizes, Patrick Collinson's 1966 study is neither recent nor particularly controversial, and clearly I did not refer to it. Collinson's views have altered in the last few years to the point that he now argues the virtual impossibility of distinguishing between "Puritans" and "Anglicans" (or "Conformists") in the Church of England. This view is extreme and does not "command almost universal assent," as a perusal of the scholarly literature of the last decade or two reveals.

Professor Russell's suggestion that I dismiss almost all work done in England is, of course, a rhetorical outburst without foundation, as he knows. The several thousand pages I have published reflect great respect for and reliance on modern British scholarship. The tragic notion of a war between English and American specialists of the Tudor-Stuart period exists only in Russell's mind. It is unfortunate that both Russell and Collinson have tried to cloak revisionism in the Union Jack, for if anything undermines constructive historical dialogue, it is the assertion of one's views in such a nationalistic context.

It is equally regrettable to see some of the revisionists brush aside the work of their critics as

merely old-fashioned. In point of fact, the New Whigs, as a few revisionists have occasionally called us, have learned much from revisionist studies, even as we have tried to forge beyond them. By retaining the valid insights of earlier historians and linking them to the newest discoveries, we seek a more balanced perspective and a more accurate understanding of the Tudor and Stuart periods. Professor Wallace's book is a major contribution to this endeavor, precisely because, in breaking new ground, he has learned from revisionists such as Professor Collinson even while demonstrating the fallacy of his recent position on "Anglican" and "Puritan."

RICHARD L. GREAVES
Florida State University

TO THE EDITOR:

When I read a review of any book, I hope to find an accurate summary of the book's contents, a critical evaluation of the book's methodology, argument and conclusions, and a discussion of how the book fits into existing literature on the subject. Manfred Jonas's review of my *Class, Culture, and the Classroom: The Student Peace Movement of the 1930s* (AHR, 88 [1983] 207-08) fails to do this. Further, it includes several assertions and a tone of hostility that I find surprising in a scholarly review.

Some of these statements are clearly contradicted by current research in the history of education and student activism. For example, the reviewer describes as a "gross generalization" my statement that in the 1920s and 1930s the "American campus both reflected and perfected the intolerance and bigotry of American society" (p. 97). His statement that this and other assertions are made "with little or no evidence" is interesting considering that the statement is made in the context of a chapter devoted substantially to discussing antisemitism and racism on American campuses in the 1930s. In addition to my own research, I cited a number of studies, including those of John Higham, Oscar and Mary Handlin, Harold Wechsler, and Marcia Graham Synnott that document discrimination in admissions, in faculty hiring and promotions, and among the fraternities, sororities, and clubs that were part of the previous student culture the activists rejected.

A similar comment was his expression of "disappointment" that a large part of the study deals with the American Student Union. Although, as I pointed out, there certainly were other organizations involved in the student peace movement, after its formation in December 1935 the ASU was clearly the major group in terms of membership, total number of campus affiliates, and influence. The other studies dealing with the thirties movement—

Philip Altbach's *Student Politics in America*, Ralph Brax's recent *The First Student Movement*; and works by Seymour Lipsett and Lewis Feuer, written from differing perspectives—attest to the major role of the ASU.

I agree with the reviewer that a study "clearly establishing the numbers, characteristics, and distribution of the students involved, the influence of the movement on and off campus, the degree to which it existed as an entity separate from other political movements," and the like would be interesting. Brax's book does much of this, but, if Professor Jonas would like to write another such book addressed to those questions, I would be happy to read it and to review it. In fact, I did touch on each of those points, but they are not the focus of the book and do not speak to the questions I was asking—about the relationship of the movement to progressive education, to the social gospel, and to Marxism and about the role of women and of Catholics and other religious groups in the movement. In addition to looking at the activism at Harvard, Columbia, CCNY, and the University of Pennsylvania, I was especially interested to see the movement at teachers' colleges, black colleges, and women's colleges. The questions may not interest the reviewer, but they are real questions nonetheless and might well be of interest to readers of the *AHR*. I object to the idea that there is only one correct set of questions to ask about social movements.

Finally, I find it peculiar for the reviewer to assert that it was "at least disingenuous to cite as the sole example of former student radicals coming under attack after World War II, the case of the Rosenbergs and Sobel." This is an odd statement, considering that the Rosenbergs and Sobel are cited as part of a five page discussion (pp. 240–45) of what happened to former student activists during the period of McCarthyism. The reviewer somehow overlooked my discussion of Reed Harris and George Edwards and James Wechsler (and Katherine Graham and David Dellinger and others perhaps too obscure for his notice).

My book was not intended as merely a factual description of now far off student antics but as a discussion of the place of students in American culture and the relationship of higher education to the American economic and social system. Some of my analysis is certainly open to question and debate. Some of it, too, is likely to arouse some anger since, especially in the concluding chapter, the analysis does, as students in the 1960s did, question the values and validity of institutions of higher education and their relationship to war and to the state. Other reviews of my book elsewhere deal with these issues. I am sorry this one does not.

EILEEN EAGAN
Illinois State University

PROFESSOR JONAS REPLIES:

I regret exceedingly that Eileen Eagan finds in my review of her book a tone of hostility—which was certainly unintended—and ascribes to me anger concerning her conclusions—which I do not feel. The disappointment reflected in my review stems solely from the fact that her book is neither, its title notwithstanding, a systematic discussion of class, culture, and the classroom nor, despite its subtitle, more than a partial and rather tendentious history of the student peace movement of the 1930s.

As for the specific points raised in her letter: (1) the undoubted fact of racism and antisemitism on American campuses in the 1930s and the evidence of discrimination provided by Higham, the Handlins, and others do not by themselves support her generalization, which makes claims about the relationship between academic institutions and the society as a whole; (2) my objection is not that prominence is given the ASU in the book but rather that that organization, its members, and their internal quarrels make up almost all of it; and, (3) had Eagan dealt extensively with the student movement at places other than Harvard, Columbia, CCNY, and Pennsylvania, my lament about narrowness would have been considerably muted. I acknowledged her treatment of Milwaukee State Teachers College as something of a saving grace. But her undoubted interest in black colleges and women's colleges—or for that matter in non-Northeastern and nonurban ones—is not adequately reflected in the final product. Activism at black colleges is touched on on 5 of the 260 pages of text. Women's colleges are mentioned on 12 pages, but half of those—the only extended treatment—deal once again essentially with the ASU. In addition, my reference to the Rosenbergs and Sobel as "sole example" was a slip. They were merely the first examples cited. It is, nevertheless, "at least disingenuous" to link their conviction and execution to their ASU membership.

Though Ralph Brax has indeed written a book that fits my prescription better than Eagan's, his has other problems (some of which, incidentally, portions of *Class, Culture, and the Classroom* could help resolve). A full-fledged history of the student peace movement of the 1930s thus remains to be written—but not by me.

MANFRED JONAS
Union College

UNDER THE RULES printed at the beginning of the Communications section, letters to the editor and replies concerning reviews must not exceed seven hundred words. Although the following exchange

exceeds that limitation significantly, we have decided to publish it in view of the nature of the issue raised.

THE EDITOR

TO THE EDITOR:

In reviewing David Abraham's *The Collapse of the Weimar Republic* (AHR, 87 [1982]: 1122–23), T. W. Mason obviously assumed that it rested on sound scholarship. Unfortunately, that assumption was not warranted. Since the book is now being cited and quoted by other historians, I think it necessary to point out that Abraham's scholarship is so faulty and misleading as to defy adequate exposure in the space available here. I shall first give only a few examples of the sort of slovenliness that renders the book, at a minimum, highly unreliable. I refer to the citation in footnotes of the following: a nonexistent book (p. 157 n. 114); a nonexistent journal article (p. 121 n. 12); a letter of October 6, 1931, as evidence about reactions to a meeting that took place five days later (p. 314 n. 105); and a letter written on November 26, 1932, attributed to a man who died five months earlier (pp. 321–22 n. 130). Such comparatively trivial mistakes can, with patience, be detected by historians with access to research libraries, but these errors reveal much about the kind of scholarship that went into Abraham's book.

More difficult for other historians to scrutinize is Professor Abraham's use of archival materials. Having used most of the German archival collections cited by Abraham, I can attest that he has repeatedly subjected documents to systematically tendentious misconstruction, sometimes to the point of outright invention. Fortunately, four of the archival documents used by Abraham are in print, although he cites only their archival file numbers. Those four provide excellent test cases, since they occupy, as presented by Abraham, a key place in his interpretation. The difficulty is that they do not say what Abraham claims they do. I invite those interested in the book to confirm this for themselves by comparing the use made of the four letters on pages 320–21 of Abraham's book with the full texts, which have been published in the following places: (1) Schacht to Reusch, March 18, 1932, in Dirk Stegmann, "Zum Verhältnis von Grossindustrie und Nationalsozialismus, 1930–1933," *Archiv für Sozialgeschichte*, 13 (1973): 450–51; (2) Reusch to Schacht, March 20, 1932, *ibid.*, 451–52; (3) Schacht to Reusch, June 6, 1932, in Kurt Koszyk, "Paul Reusch und die 'Münchener Neuesten Nachrichten,'" *Vierteljahrshefte für Zeitgeschichte*, 20 (1972): 99–100; and (4) Springorum to Wilmowsky, March 22, 1932, in Ilse Maurer and Udo Wengst, eds., *Politik und Wirtschaft*

in der Krise, 1930–1932: Quellen zur Ära Brüning, 2 (Düsseldorf, 1980): 1352–54.

The use as evidence of nonexistent archival documents ranks, of course, among the gravest of scholarly offenses. I was initially reluctant to suspect it of Professor Abraham's book. When I found quotations from two documents in the Paul Reusch papers that were unknown to me, despite my extensive research in that collection, I assumed that I had simply overlooked them. Since those quotations made the documents appear potentially as important for my own research as they are for Abraham's book, I began considering a trip to Germany to see their full texts. I recently learned, however, from the archivist in charge of the Reusch papers that the two documents are not in the file cited by Abraham. From colleagues in Germany who have also used those papers I learned that they, too, have never seen such letters. I therefore wrote to Abraham, informing him of the archivist's report and requesting from him information about the location of the two documents. In his response, he assured me that his archival citation is correct, since it conforms with his notes. The issue is, however, not the existence of his notes but of the two documents. I therefore call upon Abraham to divulge, in this professional forum, proof that two documents quoted by him on pages 164–65 and 316 of his book—a letter from Martin Blank to Paul Reusch of December 29, 1930, and "instructions" from Reusch to Blank of January 2, 1931—exist now or ever existed.

HENRY A. TURNER
Yale University

TO THE EDITOR:

In my review of David Abraham's book, *The Collapse of the Weimar Republic: Political Economy and Crisis* (AHR, 87 [1982]: 1122–23), I wrote that the author sustained his argument throughout the book "by effectively chosen quotations from a wide variety of primary sources." I did not check any of these quotations before I wrote the review. Henry A. Turner has now identified a number of passages in this book where there are discrepancies between Abraham's text and the sources which he cited. I have examined four of the relevant documents, which concern the nature and extent of big business support for the Nazi Party in 1932. The discrepancies are serious. Since Professor Abraham's book is being widely used by scholars and students who do not read German, it is necessary to present the corrections here in detail. (For the sources, see Turner's letter, above.)

The exchange of letters between Schacht (ex- and future President of the Reichsbank) and Reusch (managing director of the Gutehoffnungshütte) is

summarized by Professor Abraham on page 320. It is not immediately obvious from the way in which these documents are presented whether the author is quoting from them or paraphrasing them. In fact two of the clauses in the summary presentation are direct quotations. Schacht did write on 18 March 1932, "it will not be possible . . . to circumvent the National Socialist Party." And Reusch did respond on 20 March, "I agree fully and completely with your suggestion."

For the rest, Professor Abraham explicitly imputes motives to the two men for which the texts of their correspondence furnish no evidence. Schacht wrote nothing at this time that implies that he considered the Nazis "the positive force" or that "we should contribute to them and their efforts." The suggestion with which Reusch agreed concerned the establishment of an office that should influence the economic policies of the Nazi leadership, make these policies acceptable to "industry and trade," and "prevent dangerous nonsense" (*Unfug*) in this sphere. Schacht thought there was a good chance that this behind-the-scenes operation would succeed; if it failed, industry and trade would know that they could not rely on the Nazi party.

Reusch's concern at this stage was to persuade Hitler that his party's program in respect of economic, financial, foreign, and domestic policies should be worked out by men with the requisite expertise; in the selection of these men, their membership of the party should be a secondary consideration. Reusch put this to Hitler in a two-hour conversation on March 19 and wrote to Schacht on March 20 that Hitler had agreed. Professor Abraham is justified in describing the conversation as "productive," although Reusch did not use the term himself. There is, however, nothing in this letter (although there may be something in another letter) to indicate that Reusch found himself "in complete sympathy with the National Socialists, though they are a bit tactless."

The third document, Schacht's letter of June 6, deals with the way in which he himself, four industrialists and two bankers will share the costs of the office, which was then about to begin its work of influencing the economic program of the Nazi leadership. Professor Abraham's statement that this letter speaks of "supporting" the Nazis is incorrect.

The fourth document, Springorum's letter to five leading industrialists of March 22, 1932, is indeed about subsidies for political parties (in the context of the coming Prussian elections). What Prof. Abraham presents on page 321 as a quotation from this document, however, is in fact an incomplete and in one respect misleading paraphrase. Nowhere did Springorum say that industrialists should give "gen-

erous" financial support to the NSDAP. He described the agreed common goal of the Ruhr magnates as "the creation of a strong party of the right," which should exclude the "intransigent part of the NSDAP." He implies that industry's financial leverage may be used to split the Nazi movement. He urges his readers to overcome their distrust of Hugenberg in order that the DNVP may play a powerful role in any new party, or in any coalition government of the right; in the latter case the more moderate Nazis should be subjected to the discipline and responsibility of exercising state power.

The cumulative effect of these miscitations is to exaggerate both the enthusiasm and the practical support of one section of big business for the Nazi party. The point at stake is an important issue of interpretation which has been widely debated. Professor Abraham has got some significant details wrong. To the best of my knowledge no reviewer of the book has so far pointed out and corrected these errors. They need to be corrected.

Proven errors of this kind have done a lot to discredit Marxist and neo-Marxist interpretations of fascism and Nazism. In this type of inquiry it is exactly in specifying the relationship between economic power and political power that the greatest precision is called for. Professor Abraham's portrayal of Schacht, Reusch, and Springorum as positive supporters of the Nazi movement threatens to drag the argument back into the old morass of empirically wrong and theoretically primitive assertions concerning the relationship between economic and political power—the morass in which the NSDAP used to appear as the political instrument of capitalist interests. This would be a bitter irony indeed, for the bulk of Abraham's book represents a sustained (and, in my view, in good measure successful) effort to raise the argument to a higher plane of structural analysis—to a level, that is, at which the motivations and the choices of individuals are secondary in the sense that they are heavily determined by economic and institutional pressures. Furthermore, in terms of his own analysis, these constraints and pressures help account for the collapse of the Weimar Republic but not for the construction of the Nazi dictatorship. Thus the four passages of paraphrase and quotations are gratuitously misleading. The political motives and the attitudes toward Nazism that Abraham ascribes to Schacht, Reusch, and Springorum are in an elementary way factually incorrect. But whatever their attitudes and motives may have been, they are secondary in the author's own scheme of interpretation.

T. W. MASON
St. Peter's College
Oxford University

PROFESSOR ABRAHAM REPLIES:

Henry A. Turner raises extremely serious charges against my work. He attempts to put in question both the reliability of that work and, I fear, my own more general commitment to meeting the demands of serious historical scholarship. I consider his charges grossly misleading and substantively false. There are errors in my book, but they are of neither the kind nor the number that he attributes to me. The mistakes made are technical and not unusual; they are limited in number and surely do not render the book "highly unreliable." The documents I have used most certainly do exist and have not been fabricated. Differences in the interpretation of particular pieces of evidence will of course remain, no matter how precise our renderings.

I organize my reply in three sections. In the first, I take up Professor Turner's more specific allegations about nonexistent books, articles, documents, and the like. In the second, I respond to the questions and concerns raised by T. W. Mason and, in doing so, briefly consider those important matters of substance Turner has so resolutely chosen to avoid. Third, and last, I address Turner's more general charge that I have subjected archival documents to "systematically tendentious misconstrual."

I. Professor Turner makes five specific charges of falsification. (1) A "nonexistent" book. In the relevant footnote, I cited Carl Böhret, *Institutionalisierte Einflusswege der Verbände in der Weimarer Republik* (Berlin, 1966), 103–116, and, in the bibliography, under this entry, I indicated the publisher as Duncker und Humblot. Author, place of publication, date of publication, publisher, and page citation are all correct. Unfortunately, the title given is wrong. The correct title is *Aktionen gegen die "kalte Sozialisierung," 1926–1930*. I took the mistaken title from Hans-Josef Varain, ed., *Interessenverbände in Deutschland* (Cologne: Kiepenheuer und Witsch, 1973), in which a chapter of Böhret's book is reprinted under the title I gave. This does not, of course, excuse the mistaken citation. But I do think that any serious scholar, working with the information I provided, could have found the book. I do not know if Professor Turner made this attempt before making his accusation of fabrication.

(2) A "nonexistent" journal article. Once again, Professor Turner's accusation is technically correct and substantively wrong. The citation in the book is to an article by Richard Tilly on the place of banking in German industrial growth from the *Journal of Economic History*, 36 (1976): 180–88. The correct citation would have been to the *Journal of Economic History*, 36 (1976): 416–24. I was familiar with this piece before its publication, but it appeared finally

with Tilly as the "junior" coauthor, along with his student Rainer Fremdling. Here the citation error may be more serious, but still this is an article by the same author (as coauthor) in the same volume of the same journal. Is this a "nonexistent" article? Is it particularly difficult to locate from my reference?

(3) A "letter of October 6, 1931, as evidence about reactions to a meeting that took place five days later." Here Professor Turner is right, up to a point. The citation ought to have been to the same rapporteur's letter of October 12, 1931, which lies immediately behind the letter of October 6 in the archival file. Of course, it is not an arcane fact that the Bad Harzburg conference took place on October 11. As I was familiar with both letters (the letter of October 6 discusses expectations for the meeting), I should have caught the mistake in proofreading. But Turner must also know of the existence of the October 12 letter, since in his letter, above, he cites Maurer and Wengst, *Politik und Wirtschaft in der Krise, 1930–1932*, where the October 12 letter is published on pages 1039–43.

(4) A "letter written on November 26, 1932, attributed to a man who died five months earlier." As published in the East German collection I cited in my footnote (I was denied access to the file containing this letter in the East German archives), the author of the letter in question is identified only as a "Dr. Scholz," with no first name given. (For another reference to the same document, also not providing a first name, see Reinhard Neebe, *Grossindustrie, Staat und NSDAP, 1930–1933* [Göttingen: Vandenhoeck & Ruprecht, 1981], 260 n. 121.) At the time, the only "Dr. Scholz" I knew of who was sufficiently prominent to be included in the sort of meetings described in the letter was Ernst Scholz, former chairman of the Deutsche Volks Partei (DVP). This was the source of my incorrect inference on the authorship of the letter, a mistaken procedure that I do not here defend. The quotation, however, is correct, and the letter's significance is undisturbed by correcting its author's identity. Indeed, from discussion with Dr. Neebe, it now appears that the Dr. Scholz in question was the press spokesman for the prominent industrialist and banker Otto Wolff. If so, then even Professor Turner has failed to put his finger on the elusive Scholz, whom he has identified in a recent paper as an "agent" of the chancellor's commissar for Prussia.

(5) "The use as evidence of nonexistent archival documents." In this, his most serious charge, Professor Turner alleges wholesale fabrication of archival material. There are two documents in question. When Turner first queried me for proof of their location, he knew very well that the files in question have not been published, that no photocopying or

microfilming is permitted at the archive in question, and that the files are not even paginated. There was, therefore, no way to "prove" conclusively what he demanded proven, short of going to Germany and photographing the documents.

So I went back to the archive and photographed the documents. The two documents do indeed exist and are still located about one inch from the bottom in the file I cited. Let us consider them separately. First, there is a twelve-page report. Nowhere did I refer, as Professor Turner alleges, to "a letter" but only to "the report." As the archivist informed me, Turner asked him only to look for a personal letter, a different kind of document from what I cited and one he therefore could not find. This report lists no author and carries, in longhand only, the date "Dez 1930"—thus creating a problem of identification that ought to have been reflected in my footnote. Through an ambiguity in my transcription, which included reference to both Martin Blank and Edgar Jung, I came to identify the former as the author of the report. I now think its authorship more likely belongs to the latter. (Whereas Blank was Reusch's chief employee in Berlin, Jung was an adjunct employee of Reusch's in Munich.) I should note, however, that the authorship of this little-known December report is susceptible of several interpretations, and, quite apart from the present exchange between Turner and myself, there is now some scholarly disagreement about it. As to its dating, I inferred from internal references in other letters of Reusch that the report was sent to him on December 29, but my recent reperusal of the file suggests the report probably arrived in the week preceding. (I might note that in personal correspondence, Turner has himself dated the "nonexistent" document December 9; this is not sustained by my archival examination of the document, of which I now have a photograph.) This brings us to the second item in dispute. Reusch's acknowledgment of the report and his explicit instructions on what to do with it—namely, exactly what I wrote—are, in fact, not to Blank, who is perhaps his most frequent correspondent throughout this file, but to Jung.

Professor Turner has used an ambiguity of authorship in the first document and a mistake in identifying the recipient of the second to accuse me of the most serious of academic crimes—that of inventing nonexistent documents. Readers can judge for themselves the propriety of making such an accusation, now that all of the evidence is before them.

II. Professor Mason raises some interesting questions of presentation and interpretation, and some—but only some—of his conclusions are compelling. My decision to employ a paraphrase format for the exchanges between Schacht and Reusch (p. 320), for example, was clearly not a good one. For

readers who have not seen my book, my paraphrase of their exchange is indented in a way that for many readers suggests direct quotation. To make matters worse, some of the words employed in the paraphrase were direct quotations and should, therefore, have been put in quotation marks. The format used for the paraphrase is apparently acceptable in certain kinds of publications. In a letter on the matter that defends the format choice, Princeton University Press has stated that "there is no standard editorial style for treating a give-and-take exchange between two speakers or writers that is a paraphrase." But even if the format used is acceptable in some publications, in this case it has clearly created confusion, and I should have foreseen this when I went through galleys (especially since this was my sole usage of the paraphrase format). In any event, I will correct it in the next printing.

With respect to the substance of the paraphrases, Professor Mason offers cautions with which I can partially agree. In regard to the exchanges between Schacht and Reusch, I should have made clearer than I did in the accompanying footnote the institutional form taken by what I described as the "industrialists' efforts at 'enlightenment' " of the Nazis on economic issues—Schacht's *Arbeitsstelle*. That Schacht characterized the Nazis as "the positive force" was largely my inference. The "rightward movement in German politics proceeds irresistibly," wrote Schacht, while noting at the same time that Hugenberg's influence with the Nazis was apparently declining. I used the term "positive" rather than resting the case with Schacht's observation that the Nazis were the force to be reckoned with. But, whatever the possible deficiencies of the paraphrase, I stand by the interpretation. I think that Schacht's proposal to Reusch and several others—to organize and finance a two-year commitment, in steady touch with Hitler's appointees, to formulate for the Nazis an economic policy in accord with their own—*was* a contribution, however self-interested, to the increased power and legitimacy of the Nazi movement.

Professor Mason also questions my paraphrase of Schacht's letter of June 6, in which Schacht asked for the money for the *Arbeitsstelle*, whose work of enlightenment was, in conjunction with the two Hitler appointees, about to begin. I wrote that the funds requested were "for the purpose of supporting them and enlightening them [the Nazis] on economic issues." The attempt to reach a shared position on economic matters—through funding a bureau whose explicit purpose, as Mason himself notes, was the formulation of an economic policy to make the Nazis acceptable to critical sections of industry—is certainly support as well as enlightenment. Whether or not one cares for the word "supporting," that meaning is there: not to recog-

nize it would turn Mason's cautions into mystifications.

This issue, however, can be pursued somewhat further. In substance, support need by no means take the form of money. In April 1932, for example, during the interim between Schacht's two letters to him, Reusch instructed newspapers of which he was part-owner to cease criticism of Hitler and to promote (*fördern*) government coalitions with the National Socialists—a gesture that the Nazis clearly and correctly understood as support (see my book, 170; and Koszyk, "Paul Reusch," 92, 95, 100). Earlier, Reusch had encouraged contacts in the DVP to undermine the Brüning government—the only government then capable of mustering a majority without the Nazis. As early as August 1931 he had written the chairman of the Bavarian People's party, strongly discouraging disagreements between that party and the National Socialists on the ground that "differences within the national circles" should be "avoided." Did these actions assist the Nazis or not?

Much the same holds for Schacht's *Stelle*. His design to organize and use cooperation to bring at least Hitler's part of the NSDAP over to the economic conceptions of these Ruhr industrialists may or may not have played a key role in subsequent developments. How straight a line may be drawn from Schacht's *Arbeitsstelle* of 1932 to his establishment of the Adolf Hitler Foundation of German Industry less than a year later is a matter I leave to specialists in that field. Unlike Koszyk, Stegmann, and Turner, I did not take any particular position on these matters in my book. Indeed, I indicated on the same page that the Nazi economic platform remained ambiguous and that, although these industrialists considered enlightening the Nazis a necessary and promising approach, there was no reason to assume that it would bear fruit. Also on the same page of the text, I in fact proceeded to list the obstacles (including the radicalism of some Nazi leaders and their mass following, and the party's commitment to state interventionism in price setting, taxes, industrial administration, and trade). For his part, however, Schacht wanted as far as possible to assure (*sicherzustellen*) that "the economic policy conceptions emerging from the *Arbeitsstelle* be in harmony [*in Einklang sich befinden*] with those views represented from the national socialist side" by Hitler's two appointees. The last of the three-sentence statement from the Springorum document alone is incorrect. I would agree that the word "generous" is unjustified by the text and superfluous to my argument. To Professor Mason's presentation I would add only that Springorum called the strengthening of the DNVP a necessary roundabout way (*Umweg*) for winning the cooperation of the NSDAP in the Prussian house. This was the goal of these businessmen and their representatives, and

in pursuing it they gave no thought to the possibility of being burned by the fire they sought to harness.

Professor Mason argues from his criticisms of my paraphrases to the suggestion that I have simply exaggerated these men's support for the Nazis. Such a conclusion may appeal to Professor Turner, but it is too simple in light of the evidence. What exactly is one to think Reusch meant when, for example, he wrote to Schacht on September 21, 1932 (that is, three months after Schacht had begun organizing the *Arbeitsstelle*) that, despite his critical attitude toward and refusal to run along behind the current Papen government, he could "make no secret of the fact, that the National Socialists, toward whom I was thoroughly sympathetic, have been very disappointing these last weeks. Through their cooperation with the Communists they have deprived themselves of considerable sympathy, quite apart from the other instances of maladroitness that they have allowed themselves." ("Maladroitness" is superior to "tactless," the word I used in the book; see Reusch to Schacht, September 21, 1932, Historisches Archiv der Gutehoffnungshütte, Oberhausen/Nachlass Reusch 400101290/33a.)

Examples of such ambivalent support are one strand that runs throughout the documentation, while other strands convey different, less ambiguous images. Thus, only two days earlier, Erich von Gilsa, Reusch's confidant and former Reichstag delegate, and a man who shared Reusch's Berlin office, wrote to Reusch complaining of the demagogic and quasi-proletarian radicalism adopted by the Nazis ("even by highly respectable people like Epp and Göring") in current election campaigning. Gilsa considered it unfortunate that "leaders of industry at this very time stand up for the Nazis, rather than saying clearly that they won't take part in the new line. One industrialist, whose name was not provided me, said 'too much money has already been invested in the Nazis for us to be able to abandon them financially now.'" (Gilsa to Reusch, September 19, 1932, HA GHU/NL Reusch 400101293/4b. For more on von Gilsa, see my book, 170; and Bernd Weisbrod, *Schwerindustrie in der Weimarer Republik* [Wuppertal: Hammer Verlag, 1978], 467–74.) If we can take seriously the judgment of a widely respected senior scholar, then not only did various groups of Weimar industrialists seek a return to labor relations of a prewar kind, but they also "provided massive financial assistance to the Nazis before the November 1932 elections." "Once Hitler made clear that he had no intention of undertaking any fundamental changes in the property structure of German industry, entrepreneurial worries about socialistic tendencies could recede; then, in a certain sense voluntarily and in a certain sense compelled, the switch from the Papen-horse to the Hitler-horse took place." (Hans Mommsen in Hans Mommsen *et al.*, eds.,

Industrielles System und politische Entwicklung in der Weimarer Republik [Düsseldorf: Droste Verlag, 1974], 944.)

As I made clear several times in the text, my book is not at all centrally about relations between individual businessmen and the Nazi party. And to the extent that the text does address this issue, I of course did not wish to exaggerate the enthusiasm or practical support for the Nazi party by these or any industrialists. Nevertheless, in the end the practical support that redounded to the Nazis from the actions and choices of big business from 1930 on was considerable. Just how considerable is, of course, an important enough question to have occupied scholars for half a century. But it is difficult for me to believe, at this late date, that the existence of support can still be in question. Whether they intended them as such or not, the cumulative actions of Schacht, Reusch, Springorum, and others did lend real and tangible aid and assistance to Nazi legitimacy and success. To assert as much strikes me neither as “empirically wrong” nor as “theoretically primitive,” and to declare the issue moot with some background remarks on theory-building is hardly a more promising approach to getting at the truth.

As ever, of course, the picture is more complex. As I pointed out in the very passage on pages 320–21 that worries Professor Mason, neither Krupp nor Silverberg, two of the five recipients of Springorum’s letter, cared particularly for either Hitler or the Nazis—although the recent work of Reinhard Neebe (*Grossindustrie*) may lead all of us to revise our opinion of Silverberg downward (see my review, *AHR*, 87 [1982]: 1414, 1415). In any event, I do think most of the evidence compels us to conclude that, however individual businessmen may have felt about the Nazis, Weimar’s industrialists ultimately found themselves in a position from which it appeared that the Nazis were the best of the choices available. Passion and enthusiasm, although they may worry Mason, are in fact not particularly pertinent matters. More to the point, nowhere did I or would I argue that capitalists created the Nazis, tried to make of the Nazis their agents, or themselves brought the Nazis to power. Such assertions constitute the “old morass” of which Mason writes. I am not within it. The need to work together issued out of the necessity of stubbornly defined interests and in view of the Nazis’ own popularity. This does not mean, nor do I think, that any particularly significant industrialists liked National Socialism or looked forward to exclusive Nazi rule; possibly neither was the case.

What were the personal motivations and dispositions of Weimar industrialists? That is the topic of Professor Turner’s ongoing research, and I look forward to seeing the results. In the meantime, Professor Mason has rightly observed that in my own analysis of the republic “the motivations and

choices of individuals are secondary in the sense that they are heavily determined by economic and institutional pressures.” Such an approach does not deny all significance to individual volition, but it does seek to underscore constraints under which volition is exercised. The political alternatives that Weimar industrialists came to face were not directly of their own making, and the Nazi groundswell was something they had to address. Before 1933 there were probably not many who supported or followed the Nazis out of conviction, even among the most socially reactionary Ruhr magnates. What is critical, however, is that after a certain point there was simply no *acceptable and feasible alternative* to the NSDAP. No force other than the Nazis could claim real popular support while also demonstrating a credible commitment to eliminating Weimar’s fragmented political democracy and generous social welfare system. Industrialists wanted class peace, a free economy, and a reascendant Germany: given the stubbornness, strength, and commitment of social democracy to the republic, “in order to protect their social dominance,” Germany’s elites willingly “exposed themselves to a potentially uncertain future” (p. 326). Just as Germany’s industrialists—without any enthusiasm for the socialist movement or ideology, but in the absence of acceptable alternatives—collectively compromised with the socialists in 1918 in order to maintain the capitalist order, so by 1932 they were prepared to do the same with the Nazis.

III. Last but not least, I come to Professor Turner’s allegation that I have repeatedly submitted archival documents to “systematically tendentious misconstrual.” Turner is apparently persuaded he “can attest” to this practice on my part because he has often worked with the same documents. But as almost every historian knows, it is quite possible to use and interpret identical documents in different ways, and thus no such simple attestation is authoritative. At the core of my book is a set of fundamental questions about the structure and stability of the Weimar regime. I offered specific answers to those questions, answers with which Turner quite obviously disagrees. I have little problem with the fact of disagreement. Indeed I would welcome its clarification as a source of further historical insight. But it must be acknowledged as well that that substantive disagreement goes to the very heart of issues of interpretation, and those issues therefore cannot be resolved by appeal to some purely neutral technical principles of disinterested scholarship. Equally, I approach the questions I ask in my book with a specific set of methodological tools and presumptions, again quite different from Turner’s own. This too provides a ground for substantive disagreement, and here too neither the dimensions of that disagreement nor its possible relevance to understanding what happened during a certain period in

modern German history are usefully illuminated by aimless invective.

To flourish, the community of historical scholarship requires the freest possible exchange of divergent views. A commitment to academic pluralism must thus precede all other scholarly conclusions. Professor Turner is free in his long-awaited work on big business and the Nazis to pursue the historiographical and methodological traditions he prefers. But I hope that he can recognize that other scholars are equally free to pursue their own. And if there is a *Methodenstreit* lurking here between what one might call positivists and conceptualists, then let Turner pursue it at that level, rather than by exploiting a few errata in an attempt to discredit a detailed and comprehensive argument without confronting it. In this regard it is not irrelevant that both my empirical investigation and my conceptual framework openly employ Marxist analysis. (Perhaps this is why my book was attacked as capitalist "apologetics" by the official East German historical journal, the *Zeitschrift für Geschichtswissenschaft*.) Turner has asserted elsewhere that orthodox and nonorthodox Marxist writings alike "suffer . . . from over reliance on questionable, if not fraudulent scholarship, and from egregious misrepresentation of factual information." In his view, unless they mend their ways, "they cannot expect their position to receive a full hearing in the forum of international scholarship." (H. A. Turner, ed., *Reappraisals of Fascism* [New York: New Viewpoints, 1975], xi.) Remarks of this sort do not inspire confidence in his tolerance for a plurality of scholarly traditions. If the historical house is to have many rooms, it is inadvisable to seek to bar the door.

To "decide" the "correctness" of our competing interpretations, Professor Turner and I would have to explore more fully our points of substantive disagreement. There is no short-cut available, and I remain profoundly skeptical of his efforts to find

one. The pointlessness of his attempts to monopolize historical accuracy and truth is paradoxically illuminated by the very sources to which he now directs us, for Stegmann and Koszyk, discussing the very documents at issue, sharply contest Turner's own interpretation of them (see, for example, Stegmann, "Zum Verhältnis," 426 nn. 129, 135; and Koszyk, "Paul Reusch," 80 n. 16, 99 n. 73, and 103 n. 84). The fact of this contestation suggests again what I have attempted to insist upon all along, namely that there are important scholarly issues here, issues about which equally dedicated scholars can strenuously disagree. I hope that at some point soon we can again discuss these issues with the sobriety they deserve.

DAVID ABRAHAM
Princeton University

ERRATA

The following change should be made to my review of James E. Sargent, *Roosevelt and the Hundred Days*, in *AHR*, 88 (1983): 494, column 2, line 46: "Rosen promises . . ." to "Sargent promises. . ."

OTIS L. GRAHAM, JR.
University of North Carolina,
Chapel Hill

Please note the following price change for Hugh A. MacDougall's *Racial Myth in English History: Trojans, Teutons, and Anglo-Saxons*, incorrectly given in the *AHR*, 88 (1983): 679. The prices should read as follows: \$15.00, cloth, U.S., \$14.00, cloth, Canada, \$6.95, paper, U.S.

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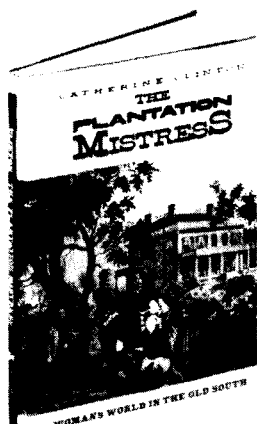
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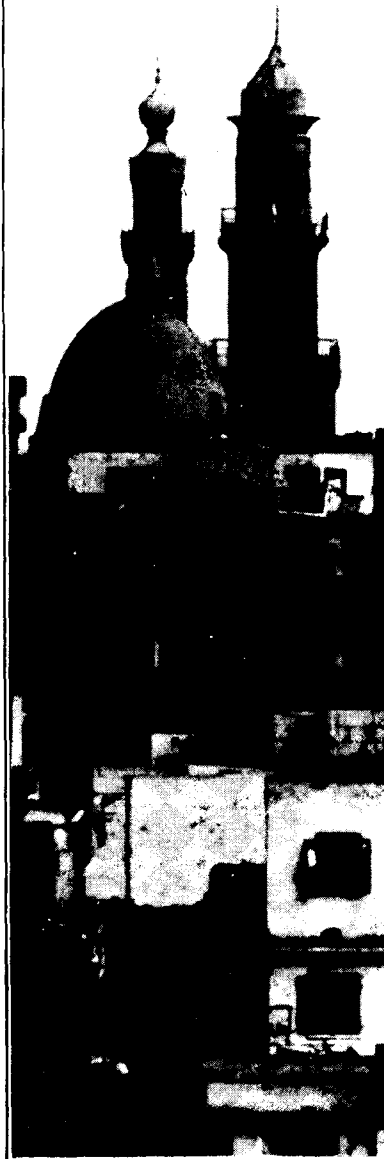


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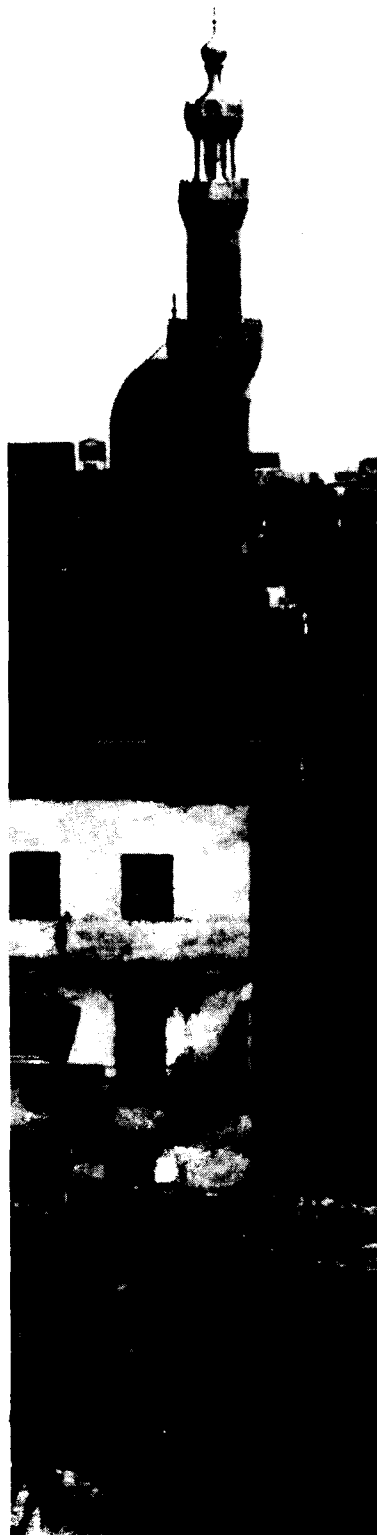
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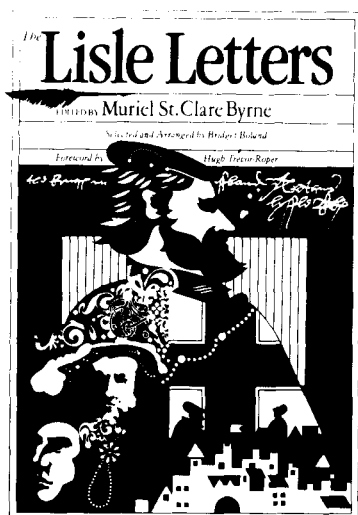
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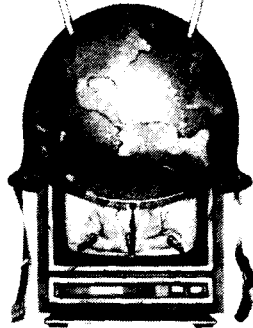
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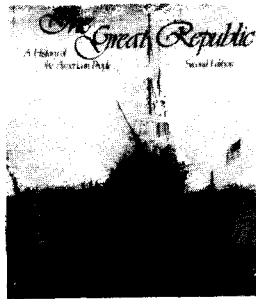
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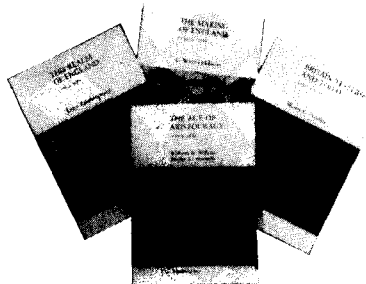
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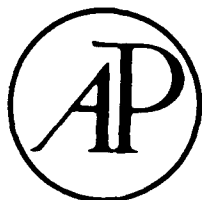
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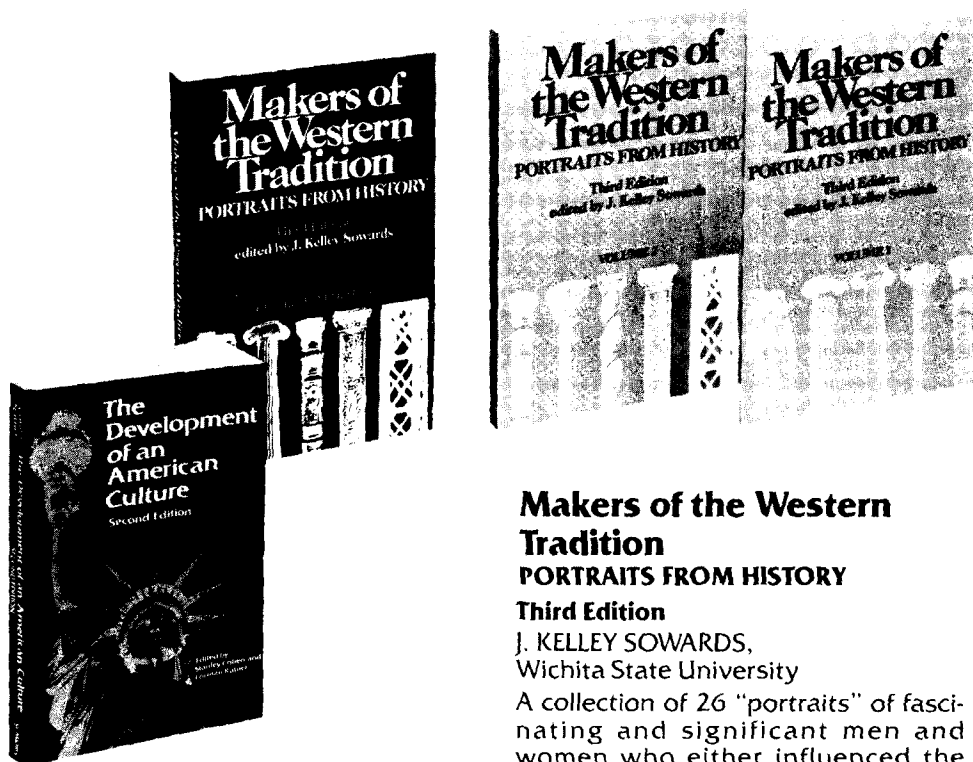
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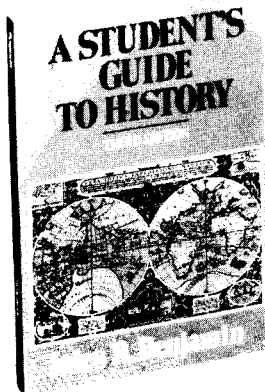
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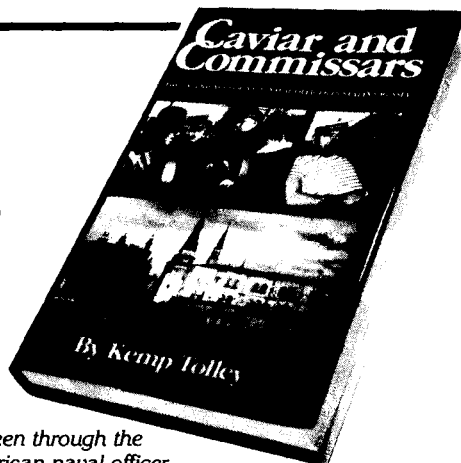
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

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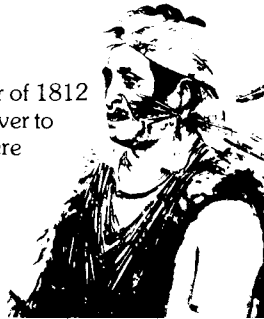
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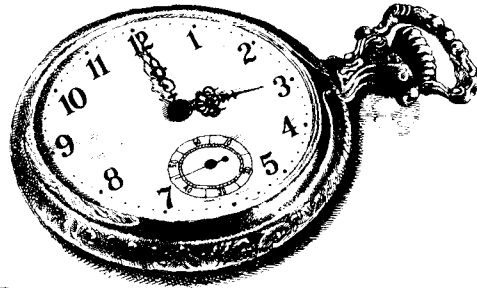
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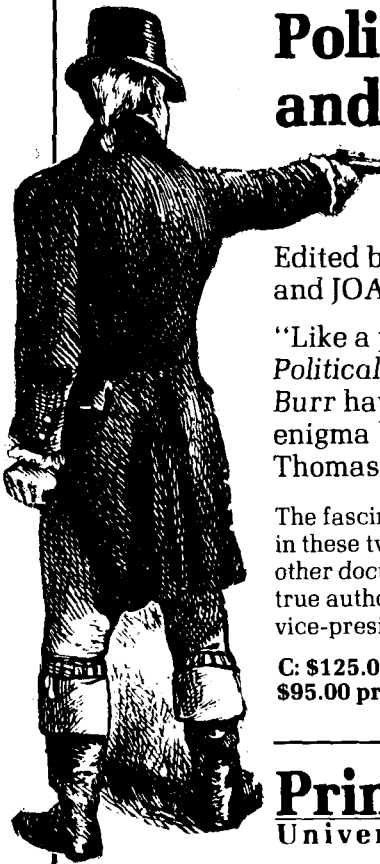
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